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ART. I.—*On the Idea of Physical and Metaphysical Infinity;* by Lieut. E. B. HUNT, Corps of Engineers, U. S. A.

FEW subjects of reflection have engaged the meditative energies of so large a portion of the leading intellects of all ages as that great idea which under a vast diversity of forms and manifestations is expressed by the word infinity. So true is this, that the charge of rash confidence would naturally arise against whoever should now profess to contribute any great additional light where so much thinking has already been expended. In spite of this presumption, I shall venture some suggestions towards a precise definition of the idea of infinity, which have served to make clearer to my own mind what before was vague and indefinite.

It has seemed to me a correct criticism on the usual modes of considering the subject of infinity, that they regard it too excluclusively under its metaphysical or speculative aspects, and too little in its physical or actualized forms. By at once pushing the idea of infinity into its abstract phases we banish it from our positive cognizance and relinquish the aids which nature affords in interpreting it to our finite comprehension. That such a hasty transfer from the concrete to the abstract form of contemplation involves a fault, may be appreciated at once by a simple consideration in which all healthful minds will doubtless agree. The idea of infinity must dwell in the divine creative mind in its greatest supposable perfection; consequently its natural embodi-

ments must possess a wholesome and intellectual truth far exceeding what could originate from the abstract speculations of human mind. Therefore, whatever hints towards a due appreciation of infinity can be gleaned from the contemplation of actual created nature, will rest on a more solid basis than any unguided speculations can claim.

Whatever is intrinsically measurable may by continuous quantitative expansion or diminution, grow to such an incomprehensible magnitude that infinity becomes predicable of its value. Thus time, distance, space, number, force, or any quality of matter or mind, as hardness, temperature, light producing power, sensational perceptive capacity, intellectual comprehension, force of will, benevolence, veneration, or indeed any species of actuality which can be regarded quantitatively, may be supposed so great or so small that the human mind will call it infinitely great or infinitely small. No such predicate would ever be used in speaking of what was not intrinsically measurable. Intrinsic measurability is therefore a fundamental preliminary to any concrete infinity. In other words, whatever is infinite must have a unit of measure, and its infinitude consists in the relation between its aggregate quantity and its unit of quantity. Physical infinities thus fundamentally involve homogeneous physical units of measure as standards of reference. Therefore the apprehension of infinity involves an apprehension of unity as its initial point, and this is equally true whether the magnitude called infinitely great or small is physical and actual, or metaphysical and only abstractly conceivable. Hence our first necessary step is to analyze the idea of a unit of measure.

When we speak of a foot, a cubic yard, a pound, an hour, a thermometer degree, &c., the ideas expressed are among the clearest which the human mind can entertain. Our mental and moral qualities, though intrinsically capable of equally exact units of measure, being actually without well defined units, their quantitative comparisons become vague and wanting in precision. Yet we are quite as prone to speak of infinite intelligence or infinite love as infinite distance or infinite time. This mode of speech rests on precisely as real a unit of measure as in the strictly physical cases, although it is less accurately defined to our own minds. If then we take a single instance of a unit of measure for close analysis, the general results of such a discussion will reach to all the analogous cases.

Taking the unit of lineal measure as this instance, we find that the original standards among all nations whence other units of length are derived, have a pretty close general agreement. The foot, the yard, the meter, the toise, &c. are all evident derivatives from the human body, and stand in close relations to certain convenient modes of measurement by reference to this geometry of the body.

Our optical perception of perspective distances involves an habitual process of reference of all dimensions seen, first to those distances near at hand which are readily comprehended, and in turn the reference of these to the actual lineal distance between the optical centres of the two eyes. This interocular distance, or stereoscopic base line, bears the same relation to exterior visible distances that the base line in a geodetic triangulation does to the entire network of the angles. Thus when we guage the perspective of a landscape, there is a direct visual perception or sensational measurement of external distances. Still we are so habituated to this stereoscopic function of the interocular base line that we do not make it an object of conscious contemplation, in our references of external distances to this base as a unit of measure. Such a reference however really enters as a vital part of every perspective perception, hence we are constantly applying unawares a standard measure, essentially constant for each individual during his whole life, to all external objects of our earthly surroundings. In like manner the length of our habitual step enters largely as a basis in our estimation of distances because we are constantly measuring distances seen, by our habitual mechanism of locomotion. Thus all our means of knowing external distances are found at last to rest solely on the actual dimensions of the human body to which as a standard they are referred.

Taking into account all the elements of our perception of lineal magnitudes, it will be found without doubt that the average lineal unit is very nearly that length which is most readily cognizable by a man of average person and capacities. High multiples and low submultiples of the standard of length are difficult of appreciation; thus a mile or a line are much less clearly apprehended units than a foot. If a thousand miles or a thousandth of a line is submitted to our consciousness, our notion becomes extremely inadequate, and if it be a million or a millionth we fail almost entirely to conceive the fact. When it is a question of billions or trillions of miles our apprehension is so totally at fault that we give over all attempts to comprehend the fact, and so the distance becomes infinite for us, as referred to the mile as a unit. Our real idea then is when we speak of an infinite or infinitesimal distance, that when it is compared with our familiar standard of length, our perceptive powers utterly fail to appreciate the relation with any approach to accuracy.

If in place of the unity and infinity of distance, we consider those of time, space or force, we shall find a like genesis of practical standard units for each, based on the actual dimensions or sensational capacities of the human organism. Along the graduated line of connection between those values which by their minuteness utterly elude our perception and those vast values

which in their entirety wholly transcend our comprehension, there is in each case a particular value which is apprehended with the maximum precision and facility by each individual mind. The natural unit of measure for each subject of measure is that particular value which is best appreciated by an average man. The same rule holds in the more transcendental subjects of measurement. Thus a man of average intellectual power and capacity becomes a natural unit of mentality, and a man of average morality becomes a unit of morality. If a particular moral quality, as benevolence, for instance, be quantitatively considered, we refer all to the man of average benevolence. When we speak of Divine Benevolence as infinite, we mean that it is so exhaustless and all prevailing, that when we compare it with the benevolence of an average man our limited human powers utterly fail to take in its relative immensity. From these considerations we may conclude that whenever we predicate infinity of any particular existence, attribute or quality of the external world, man's capacity to appreciate the various values of the subject matter considered, enters directly as the standard of comparison. Thus to say that space is infinite is simply to say that the extreme exercise of human power to perceive space as an existence is transcended by the actuality of nature. If we speak of infinite time we but declare that the brief periods of duration of which man in his earthly life is conscious are relatively so small that we can by no means conceive the number expressing the true ratio of man's hour or lifetime to the infinite duration referred to.

Whatever physical infinity engages our consideration, an analogous limitation of the special powers of the human organism is defined. We might almost say that for us, the grand sphere of physical infinity is the circumscribing sphere drawn around the aggregate perceptive faculties of man. It is not at all the absolute cosmos or circumscribing sphere which contains all the actualities of nature. We may well believe that this true cosmic sphere in which all created existence is contained is itself an infinity as compared with that specific infinity which is as it were the defining or tangent surface around the faculties of man.

It is entirely supposable that among the actual organic existences of nature, there may be numerous successive grades of perceptive capacity which stand in the relation of coterminous or successive infinities as compared with each other. A monad may have a direct perception of man's infinitely small, and our sensible distance must be to it an infinitely great magnitude. There may be intelligences such that the radius of an ultimate atom of matter would be to them what the radius of the earth is to us, as there may be intelligences to which the earth is but an atom

and to which our entire sphere of visible stars makes but a sensible mass of matter. Throughout the entire range of organic existence there will be in fact for each species a specific infinity, and we cannot say but in the treasure house of the actual universe, there may be an infinite series of organic perceptive powers which bear to each other the relation of the successive orders of differences in differential calculus. Whatever may be the fact as to actual nature, such an infinite series of successive infinities is metaphysically conceivable. The clear apprehension of the idea of infinity which may be gleaned from physical grounds gives a basis for indefinite metaphysical fabrications without in the least departing from the true inductive idea of infinity. But science has not to deal with the supposable except as it is involved in the actual, and it belongs not to this place or to true philosophy to go beyoud the foundations of fact.

The views now presented have a bearing on the mathematical idea of infinity which is not without importance. The mathematical symbol of infinity stands for an entirely abstract idea. From it all the definite standards of unity which have been discussed are entirely eliminated, all specific intelligences are in it ignored, and even the Divine Intelligence may be supposed in some way concrete in conditions too specific to be truly stated in respect to limits, by the abstract infinity of pure analysis. The very possibility of positive definition as applied to any being however exalted, excludes the abstract symbol of infinity from entering a correct exegesis of its nature. For what then does the abstract symbol of infinity stand? It at least stands as a formula for all specific infinities which by the interpolation of the proper constants expresses the quantitive relations in any actual case of infinity. The abstract symbol is a grouping of specific cases: whether it is more than this may not be for man to say.

One inference from these views of infinity is that the ordinary definition of the asymptotic curve needs correction. The mathematical formula of incessant and incessantly diminishing approach between a straight line and a curve or between two curved lines, or the same relative to plane and curved surfaces is not consistent with the other idea of tangency at an infinite distance. Suppose an intellect of the proper or differential grade to be duly cognizant of asymptotic lines at an infinite distance; it would find no actual contact, but the same law of approach expressed in the analytical formula would still go on until an intellect of the second differential order would have to be called in as the cognizant power: this in turn must give place to a third differential intellect, and so on to infinity. The order of perceptions required to appreciate the second, third, &c. differentials of the function would be progressively higher than that demanded for the differentials of the variable. Here then there is no true tan-

gency but a perpetually decreasing approximation. Hence the definition of asymptotes should give the idea of a perpetually diminishing and never ending approximation instead of involving the false notion of tangency anywhere. The geometrical method of exhaustions escapes any such criticism and in the purest manner embodies the true conception of infinity.

As might be expected, the infinitesimal calculus expresses the notion of infinity in its most precise and purified form. The fundamental idea on which its processes and algorithm rest, is one of relation between quantities cognizable by two orders of perception, so remote that the finite quantity of the one is the infinitely great or small of the other. By the hypothecation of three, four or more coterminous orders of perceptive faculty the second, third, &c. orders of differences are philosophically originated. The relations of differential calculus if inversely stated become those of integral calculus, and fall under the same generalizations relative to orders of perceptive capacity. I will venture the suggestion that the reason why some eminent mathematicians have contended that a differential is absolutely zero, is simply because of their not having regarded, as they clearly are bound to do, the element of limited perceptive faculties which infinity involves and the consequently supposable series of cognitions precisely conforming with first, second, third, &c. differentials. It is the same fault of conception which invalidates the definition of asymptotes based on tangency at an infinite distance.

In conclusion, we may lay down the general proposition that the idea of infinity involves in all cases as an essential factor in its composition, a specific actual or hypothetical limitation of perceptive power in the intelligence of whose cognitions infinity is predicated. In the physical infinities which chiefly interest us, the limitations involved are *for us*, altogether those which encompass the mind of man. We can by hypothesis suppose other limits conformed to other grades of organisms, and we can even suppose such an exalted spiritual organism as that the absolute and entire cosmos shall be the true limit of its perceptive powers. Before the divine mind this cosmos must stand in that clear finite relation necessarily supposed between a creator and the thing created. Beyond the actual cosmos there may be an immensity of possibility where the divine mind may realize analogous limitations to those which hedge in all created minds. In the mathematical or purely abstract idea of infinity there seems a suggestion of such a possibility, and when we consider that a mathematical formula is the nearest possible approach to a literally divine thought, we shall bow with reverence before the suggestion shadowed forth by the sublime symbol of infinity after all created limitations are eliminated from its significance.

This symbol affords no basis for the commonly received idea that infinity means an absolute unboundedness, a quantity absolutely without end, a quality or nature transcending *all* boundaries. Such an idea has no right in the mind of man, for the limitations of human perception forbid our attainment of any knowledge either of the extent of the absolute cosmos or of the boundaries around what is abstractly possible. The formula of infinity, so far from stating an absolute boundlessness, endlessness or illimitable magnitude, states simply the limitations of finite perceptive power. It is the expression, not of the immeasurableness of nature or of the Deity, but of the finite limitations of the human mind. It stands for a negative and not for a positive: it symbolizes not knowledge but ignorance. If we group infinite attributes under a divine name, we have not defined Deity but we have defined the limits of our own conceptions. The limits of our knowledge lie near at hand: the limits of our ignorance are known only to the All-knowing.

ART. II.—*On the Characters, Principles of Division, and Primary Groups of the Class Mammalia;* by Professor OWEN, F.R.S., F.L.S., Superintendent of the Natural History Departments in the British Museum.*

THE class Mammalia, the most highly organized of the animal kingdom and that to which we ourselves belong, appears to have been the class of animals last introduced on this planet, and not to have attained plenary development until the tertiary division of geological time.

Mammals are distinguished, outwardly, by an entire or partial covering of hair, and (with two exceptions) by teats or mammæ —whence the name of the class.† All Mammals possess mammary glands, and suckle their young: the embryo or fœtus is developed in the womb. Their leading anatomical character is to have lungs, composed of a highly vascular and minutely cellular structure throughout, and suspended freely in a thoracic cavity separated by a muscular and tendinous septum or diaphragm from the abdomen.

Mammals, like birds, have a heart composed of two ventricles and two auricles, and have warm blood: they breathe quickly; but inspiration is performed chiefly by the agency of the diaphragm; and the inspired air acts only on the capillaries of the pulmonary circulation.

* This paper is cited from the Journal of the Proceedings of the Linnean Society of London. Read February 17th and April 21st, 1857.

† From *mamma*, a pap. The Platypus and Echidna are the only known exceptions to this rule. The Mare is an apparent one, from the pudendal position of the nipples. The fœtal Cetacea show tufts of hair on the muzzle.

The blood-discs are smaller than in Reptiles, and, save in the camel-tribe, are circular. The right auriculo-ventricular valve is membranous, at least never entirely fleshy; and the aorta bends over the left, never over the right, bronchial tube. The primary branches of the aorta are given off not immediately after, but at a little distance from, its origin, and there is less constancy in the order of their origin than in Birds: the phrenic arteries, the cœliac axis, and the superior mesenteric artery are always branches of the abdominal aorta, which terminates by dividing beyond the kidneys into the iliac arteries, from which spring both the femoral and ischiadic branches: the caudal or sacro-median artery, which in some long-tailed Mammals assumes the character of the continued trunk of the aorta, never distributes arteries to the kidneys or the legs, as in Birds. The kidneys are nourished, and derive the material of their secretion, exclusively from the arterial system. Their veins are simple, commencing by minute capillaries in the parenchyma and terminating generally by a single trunk on each side in the abdominal vena cava: they never anastomose with the mesenteric veins.

The kidneys are relatively smaller and present a more compact figure than in the other vertebrate classes; their parenchyma is divided into a cortical and medullary portion, and the secreting tubuli terminate in a dilatation of the excretory duct, called the pelvis.

The liver is generally divided into a greater number of lobes than in Birds. The portal system is formed by veins derived exclusively from the spleen and chylopoietic viscera. The cystic duct, when it exists, always joins the hepatic, and does not enter the duodenum separately. The pancreatic duct is commonly single.

The mouth is closed by soft flexible muscular lips: the upper jaw is composed of palatine, maxillary and premaxillary bones, and is fixed; the lower jaw consists of two rami, which are simple or formed by one bony piece, and are articulated by a convex or flat condyle to the base of the zygomatic process, and not to the tympanic element of the temporal bone; the base of the coronoid process generally extends along the space between the condyloid and the alveolar processes. The jaws of Mammals, with few exceptions, are provided with teeth, which are arranged in a single row; they are always lodged in sockets, and never anchylosed with the substance of the jaw. The tongue is fleshy, well-developed, with the apex more or less free. The posterior nares are protected by a soft palate, and the larynx by an epiglottis: the rings of the trachea are generally cartilaginous and incomplete behind: there is no inferior larynx. The œsophagus is continued without partial dilatations to the stomach, which varies in its structure according to the nature of the food, or the quantity of nutriment to be extracted therefrom.

The true vertebræ of Mammalia have their bodies ossified from three centres, and present for a longer or shorter period of life a discoid epiphysis at each extremity. They are articulated by concentric ligaments with interposed glairy fluid forming what are called the intervertebral substances; the articulating surfaces are generally flattened, but sometimes, as in the neck of certain Ruminants, they are concave behind and convex in front: such a vertebra, however, may be distinguished from a vertebra of a Reptile, with a similar ball-and-socket structure of the articular surfaces, even when found in a fossil state, and when the test of the articulating medium cannot be applied, by the complete anchylosis or confluence of the annular with the central part or body, and by the large relative size of the canal for the spinal chord. The cervical vertebræ, with one or two exceptions, are seven in number, neither more nor less: the Monotremes, which are the instances commonly opposed to other generalizations, form no exception to this rule. The lumbar vertebræ are more constant and usually more numerous than in other classes of vertebrate animals. The atlas is articulated by concave articular processes to two convex condyles, which are developed from the ex-occipital elements of the last cranial vertebra. The tympanic element of the temporal bone is restricted in function to the service of the organ of hearing, and never enters into the articulation of the lower jaw. The olfactory nerves escape from the cranial cavity through numerous foramina of a cribriform plate. The optic foramina are always distinct from one another.

The scapula is generally an expanded plate of bone; the coracoid, with two (monotrematous) exceptions, appears as a small process of the scapula. The sternum consists of a narrow and usually simple series of bones: the sternal portions of the ribs are generally cartilaginous and fixed to the vertebral portions without the interposition of a distinct articulation: there are no gristly or bony abdominal ribs or abdominal sternum. The pubic and ischial arches are generally complete, and united together by bony confluence on the sternal aspect, so that the interspace of the two pelvic arches is converted into two holes, called *foramina obturatoria* or *thyroidea*. The sclerotic coat of the eye is a fibrous membrane, and never contains bony plates. In the quantity of aqueous humor and the convexity of the lens Mammals are generally intermediate between Birds and Fishes. The organ of hearing is characterized by the full development of the cochlea with a lamina spiralis: there are three distinct ossicles in the tympanum; the membrana tympani is generally concave externally; the meatus auditorius externus often commences with a complicated external ear, having a distinct cartilaginous basis. The external apertures of the organ of smell

are provided with movable cartilages and muscles, and the extent of the internal organ is increased by accessory cavities or sinuses which communicate with the passages including the turbinated bones.

There are few characters of the osseous system common, and at the same time peculiar, to the class Mammalia. The following may be cited:

1. Each half or ramus of the mandible consists of one bony piece developed from a single centre: the condyle is convex or flat, never concave. This has proved a valuable character in the determination of fossils.

2. The second or distal bone, called "squamosal," in the bar continued backwards from the maxillary arch, is not only expanded, but is applied to the side-wall of the cranium, and develops the articular surface of the mandible, which surface is either concave or flat.*

3. The presphenoid is developed from a centre distinct from that of the basisphenoid.

In no other class of vertebrate animals are these osteological characters present.

The cancellous texture of mammalian bone is of a finer and more delicate structure than in Reptiles, and forms a closer network than in Birds. The microscopic radiating cells are relatively smaller and approach more nearly to the spheroid form; but both these histological characters are liable to mislead, if unsupported by more obvious and constant ones, in the interpretation of a fossil.

Dental characters.—The Mammalia, like *Reptilia* and *Pisces*, include a few genera and species that are devoid of teeth; the true ant-eaters (*Myrmecophaga*), the scaly ant-eaters or pangolins (*Manis*), and the spiny monotrematous ant-eater (*Echidna*), are examples of strictly edentulous Mammals. The Ornithorhynchus has horny teeth, and the whales (*Balæna* and *Balænoptera*) have transitory embryonic calcified teeth, succeeded by whalebone substitutes in the upper jaw. The female Narwhal seems to be edentulous, but has the germs of two tusks in the substance of the upper jaw-bones; one of these becomes developed into a large and conspicuous weapon in the male Narwhal, whence the name of its genus *Monodon*.

The examples of excessive number of teeth are presented, in the order *Bruta*, by the priodont Armadillo, which has ninety-eight teeth: and in the Cetaceous order by the Cachalot, which has upwards of sixty teeth, though most of them are confined to the lower jaw; by the common Porpoise, which has between eighty and ninety teeth; by the Gangetic Dolphin, which has one hundred and twenty teeth; and by the true Dolphins (*Del-*

* The Wombat is, perhaps the sole exception to this rule.

phinus), which have from one hundred to one hundred and ninety teeth, yielding the maximum number in the class Mammalia.

When the teeth are in excessive number, as in the Armadillos and Dolphins above cited, they are small, equal, or sub-equal, and usually of a simple conical form.

In most other mammals particular teeth have special forms for special uses; thus, the front teeth, from being commonly adapted to effect the first coarse division of the food, have been called cutters or *incisors;* and the back teeth, which complete its comminution, grinders or *molars;* large conical pointed teeth situated behind the incisors, and adapted, by being nearer the insertion of the biting muscles, to act with greater force, are called holders, tearers, laniaries, or more commonly *canines*, from being well developed in the Dog and other Carnivora.

It is peculiar to the class Mammalia to have teeth implanted in sockets by two or more fangs; but this can only happen to teeth of limited growth, and generally characterizes the molars and pre-molars: perpetually growing teeth require the base to be kept simple and widely excavated for the persistent pulp. In no mammiferous animal does anchylosis of the tooth with the jaw constitute a normal mode of attachment. Each tooth has its peculiar socket, to which it firmly adheres by the close coadaptation of their opposed surfaces, and by the firm adhesion of the alveolar periosteum to the organized cement which invests the fang or fangs of the tooth.

True teeth implanted in sockets are confined, in the Mammalian class, to the maxillary, premaxillary, and mandibular or lower maxillary bones, and form a single row in each. They may project only from the premaxillary bones, as in the Narwhal; or only from the lower maxillary bone, as in *Ziphius;* or be limited to the superior and inferior maxillaries and not present in the premaxillaries, as in the true *Ruminantia* and most *Bruta* (Sloths, Armadillos, Orycteropes). In most Mammals, teeth are situated in all the bones above mentioned.

The teeth of the Mammalia usually consist of hard unvascular dentine, defended at the crown by an investment of enamel, and everywhere surrounded by a coat of cement.

The coronal cement is of extreme tenuity in Man, Quadrumana and the terrestrial Carnivora; it is thicker in the Herbivora, especially in the complex grinders of the Elephant.

Vertical folds of enamel and cement penetrate the crown of the tooth in the ruminating and many other Ungulata, and in most Rodents, characterizing by their various forms the genera of those orders.

No Mammal has more than two sets of teeth. In some species the tooth-matrix does not develop the germ of a second

tooth, destined to succeed one into which the matrix has been converted; such a tooth, therefore, when completed and worn down, is not replaced. The Sperm Whales, Dolphins, and Porpoises are limited to this simple provision of teeth. In the Armadillos and Sloths, the want of generative power, as it may be called, in the matrix is compensated by the persistence of the matrix, and by the uninterrupted growth of the teeth.

In most other Mammalia, the matrix of the first-developed tooth gives origin to the germ of a second tooth, which sometimes displaces the first, sometimes takes its place by the side of the tooth, from which it has originated.

All those teeth which are displaced by their progeny are called 'temporary,' deciduous, or milk-teeth; the mode and direction in which they are displaced and succeeded, viz. from above downwards in the upper, from below upwards in the lower, jaw, in both jaws vertically—are the same as in the Crocodile; but the process is never repeated more than once in any mammalian animal. A considerable proportion of the dental series is thus changed; the second or 'permanent' teeth having a size and form as suitable to the jaws of the adult, as the 'temporary' teeth were adapted to those of the young animal.

Those permanent teeth, which assume places not previously occupied by deciduous ones, are always the most posterior in their position, and generally the most complex in their form. The term 'molar' or 'true molar' is restricted to these teeth. The teeth between them and the canines are called 'premolars;' they push out the milk-teeth that precede them, and are usually of smaller size and simpler form than the true molars.

Thus the class Mammalia, in regard to the times of formation and the succession of the teeth, may be divided into two groups, *monophyodonts*,* or those that generate a single set of teeth; and the *diphyodonts*,† or those that generate two sets of teeth. But this dental character is not so associated with other organic characters as to indicate natural or equivalent subclasses.

In the Mammalian orders with two sets of teeth, these organs acquire fixed individual characters, receive special denominations, and can be determined from species to species. This individualization of the teeth is eminently significative of the high grade of organization of the animals manifesting it.

Originally, indeed, the names 'incisors,' 'canines,' and 'molars,' were given to the teeth, in Man and certain Mammals, as in Reptiles and Fishes, in reference merely to the shape and offices indicated by these names; but they are now used as arbitrary signs, in a more fixed and determinate sense. In some

* *μόνος*, once; *φύω*, I generate; *ὀδούς*, tooth.

† *δίς*, twice; *φύω* and *ὀδούς*. See "Philosophical Transactions," 1850, p. 493.

Carnivora, *e. g.* the front-teeth have broad tuberculate summits, adapted for nipping and bruising, while the principal back-teeth are shaped for cutting, and work upon each other like the blades of scissors. The front-teeth in the Elephant project from the upper jaw, in the form, size and direction of long pointed horns. In short, shape and size are the least constant of dental characters in the Mammalia; and the homologous teeth are determined, like other parts, by their relative position, by their connexions, and by their development.

Those teeth which are implanted in the premaxillary bones, and in the corresponding part of the lower jaw, are called 'incisors,' whatever be their shape or size. The tooth in the maxillary bone which is situated at or near to the suture with the premaxillary is the 'canine,' as is also that tooth in the lower jaw, which, in opposing it, passes in front of the upper one's crown when the mouth is closed. The other teeth of the first set are the 'deciduous molars;' the teeth which displace and succeed them vertically are the 'premolars;' the more posterior teeth, which are not displaced by vertical successors, are the 'molars' properly so called.

I have been led, chiefly by the state of the dentition in most of the early forms of both carnivorous and herbivorous Mammalia, which flourished during the eocene tertiary periods, to regard 3 incisors, 1 canine, and 7 succeeding teeth, on each side of both jaws, as the type formula of diphyodont dentition.

Three of the seven teeth may be 'premolars,' and four may be true 'molars;' or there may be four premolars, and three true molars. This difference as I have elsewhere shown, forms a character of a secondary group or order in the mammalian class.* The essential nature of the distinction is as follows: true molars are a backward continuation of the first series of teeth; they are developed in the same primary groove of the fœtal gum; they are 'permanent' because they are not pushed out by successional teeth—the 'premolars,' called 'dents de remplacement' by Cuvier. Seven teeth developed in the primary groove is, therefore, the typical number of first teeth, beyond the canines. If, as in *Didelphys*, the anterior three develop tooth-germs, which come to perfection in a 'secondary groove,' there are then 3 deciduous teeth, 3 premolars, and 4 true molars: if, as in *Gymnura*, the anterior four of the 'primary' teeth develop tooth-germs, which grow in a secondary groove, there are then 4 deciduous teeth, 4 premolars, and 3 true molars. The first true molar of the marsupial is thus seen to be the homologue of the last milk-molar of the placental.

The Gymnure, the Mole, and the Hog are among the few existing quadrupeds which retain the typical number and kinds of

* Outlines of a Classification of the Mammalia, Trans. Zool. Soc. vol. ii, p. 330 (1839).

teeth. In a young Hog of ten months, the first premolar, *p.* 1, and the first molar, *m.* 1, are in place and use together with the three deciduous molars, *d.* 2, *d.* 3, and *d.* 4; the second molar, *m.* 2, has just begun to cut the gum; *p.* 2, *p.* 3, and *p.* 4, together with *m.* 3, are more or less incomplete, and will be found concealed in their closed alveoli.*

The last deciduous molar, *d.* 4, has the same relative superiority of size to *d.* 3 and *d.* 2, which *m.* 3 bears to *m.* 2 and *m.* 1; and the crowns of *p.* 3 and *p.* 4 are of a more simple form than those of the milk-teeth, which they are destined to succeed. When the milk-teeth are shed, and the permanent ones are all in place, their kinds are indicated, in the genus *Sus*, by the following formula:—

$$i.\ \frac{3-3}{3-3},\ c.\ \frac{1-1}{1-1},\ p.\ \frac{4-4}{4-4},\ m.\ \frac{3-3}{3-3}=44:$$

which signifies that there are on each side of both upper and lower jaws 3 incisors, 1 canine, 4 premolars, and 3 molars, making in all 44 teeth, each tooth being distinguished by its appropriate symbol, e. g., *p.* 1 to *p.* 4, *m.* 1 to *m.* 3. This number of teeth is never surpassed in the placental Diphyodont series.

When the premolars and the molars are below this typical number, the absent teeth are missing from the fore part of the premolar series, and from the back part of the molar series. The most constant teeth are the fourth premolar and the first true molar; and these being known by their order and mode of development, the homologies of the remaining molars and premolars are determined by *counting the molars from before backwards*, *e. g.* 'one,' 'two,' 'three,' and the premolars *from behind forwards*, 'four,' 'three,' 'two,' 'one.' The incisors are counted from the median line, commonly the foremost part, of both upper and lower jaws, outwards and backwards. The first incisor of the right side is the homotype, tranversely, of the contiguous incisor of the left side in the same jaw, and vertically of its opposing tooth in the opposite jaw; and so with regard to the canines, premolars, and molars; just as the right arm is the homotype of the left arm in its own segment, and also of the right leg of a succeeding segment. It suffices, therefore, to reckon and name the teeth of one side of either jaw in a species with the typical number and kinds of teeth, *e. g.* the first, second, and third incisors,—the first, second, third, and fourth premolars,—the first, second, and third molars; and of one side of both jaws in any case.

* I recommend this easily acquired 'subject' to the young zoologists for a demonstration of the most instructive peculiarities of the mammalian dentition. He will see that the premolars must displace deciduous molars in order to rise into place: the molars have no such relations.

I have been induced to dwell thus long on the dental characters of the class *Mammalia*, because they have not been clearly or accurately defined in any systematic or elementary work on zoology, although an accurate formula and notation of the teeth are of more use and value in characterizing genera in this than in any other class of animals.

I next proceed to review briefly the principal primary divisions of the *Mammalia* hitherto proposed. The best authorities in Natural History have adopted different characters, drawn from different systems of organs, for the primary groups or divisions of the class *Mammalia*.

Aristotle chose the locomotive system, and divided his Zootoka—the equivalent of the Linnean Mammalia—into three sections: 1st, Dipoda, or bipeds; 2nd, Tetrapoda, or quadrupeds; and 3d; Apoda, or impeds. The preponderating second group, which includes all the class save the Human-kind and the Whale-tribe, is subdivided into those with claws, and those with hoofs. The unguiculate quadrupeds are again subdivided according to the nature of their teeth; the ungulate quadrupeds, according to the divisions of their hoofs, as *e. g.* into *Polyschidæ*, or multungulates, *Dischidæ*, or bisulcates, and *Aschidæ*, or solid-ungulates. I need scarcely remark that this, in most respects admirable, system, would have commanded greater attention, and been now recognized as more manifestly the basis of later systems, had its immortal author more technically expressed his appreciation of the law of the subordination of characters; but he applies to each of his groups, whatever their value, the same denomination, viz. *genos*, or genus.

Ray, with a less philosophical appreciation of the extent and nature of the class *Zootoka* or *Mammalia*, arranges his equivalent group of "Viviparous Four-footed animals" chiefly on the Aristotelian characters; the primary division being into Ungulate and Unguiculate, and the subdivisions being based on locomotive and dental characters.

Linnæus, restoring the class *Mammalia* to its Aristotelian integrity, primarily subdivides it into Unguiculata, Ungulata, and Mutica, the latter being the 'Apoda' of Aristotle: the secondary groups or orders are founded chiefly on modifications of the dental system.

Cuvier, adopting the same threefold primary division of the class, subdivides it into better and more naturally defined orders, according to various characters derived from the dental, the osseous, generative, and the locomotive systems.

Illiger, in primarily dividing the Mammalia into those with free, and those with fettered limbs—the 'pedes exserti distincti,' contrasted with the 'pedes retracti obvoluti,'—made a more unequal and less natural partition than the threefold one of Aris-

totle; the Seals and the Whales balance all the rest of the class in the Illigerian system. The subdivisions, also of these primary groups, based exclusively on characters of locomotion, have met with little acceptance beyond some of the schools of Germany.

De Blainville appears first, 1816, to have adopted a character from the reproductive system for the primary division of the Mammalia, viz. into the 'Monodelphes,' 'Didelphes,' and 'Ornithodelphes.' His orders are in the main a return to the Linnean system and nomenclature, with some peculiar views, as *e. g.* of the quadrumanous or primatial affinity of the Sloths, which have never gained acceptance. But his system indicates a clearer appreciation or stronger conviction of the value of the character of parity and imparity in the number of toes of the *Ungulata*, first suggested by Cuvier,* than was subsequently entertained by the originator of the idea.

The position of the marsupial and monotrematous quadrupeds at the bottom of the class *Mammalia*, and the higher value assigned to the group which they constituted, than that in the 'Règne Animal' of Cuvier, were ideas also in closer conformity with nature. They were, however, but surmises, unsustained by anatomical knowledge; and, as such, failed to carry conviction, or gain acceptance. Nor was it until comparative anatomy had shown that the Marsupials and Monotremes agreed in differing from all other mammals in the absence of a placenta, and of the great commissure of the brain, in certain bird-like characters of the heart,† and from all other diphyodont Mammals in a less number of premolars, and a greater number of true molars,—depending essentially on the retention of a milk-tooth (*m.* 4), which is displaced and changed in the placental diphyodonts,—that the true affinities of the didelphid and ornithodelphid mammals to each other, and their true position in the class *Mammalia*, were finally recognized.

In the 'Systema Vertebratorum,' communicated in 1840 to the Linnean Society by that accomplished and indefatigable zoologist Prince Charles Lucien Bonaparte, the primary subdivision of the Mammalia according to developmental and generative characters is adopted; and the first division or series *Placentalia* is subdivided, agreeably with M. Jourdan's distribution of Mammalia in the Leyden Museum, into the two subclasses *Educabilia* and *Ineducabilia*, the latter including the orders *Bruta*, *Cheiroptera*, *Insectivora* and *Rodentia*, with the common character of 'cerebrum unilobum.' This I regard as the most important improvement in the classification of the Mammalia, which has been proposed since the establishment of the natural character of the implacental or ovo-viviparous division.

* Ossemens Fossiles. 4to, ed. 1812. p. 9; tom. iii, ed. 1822. p. 72.

† On the Classification of the *Marsupialia*, Zoological Transactions, vol. ii, p. 315 (1839).

Cuvier had early noticed the relation of the Australian mammals, as a small collateral series, to the unguiculate mammals of the rest of the world, "some," he writes, "corresponding with the *Carnaria*, some with the *Rodentia*, and others again with the *Edentata*."*

M. Isidore Geoffroy St. Hilaire, in his 'Classification parallèlique des Mammifères,' published in 1845, raises the *Marsupialia*, to the rank of a distinct class, and literally exemplifies the idea of Cuvier by placing its subdivisions, as orders, in parallel equivalents with the orders of the *Placentalia*.

It does not appear, however, that Cuvier meant to do more than indicate certain relations of analogy; just as the relation of the pedimanous and frugivorous Marsupials to the pedimanous *Quadrumana* of S. America, that of the marsupial Hyæna (*Thylacinus*) to the Wolf, of the Flying Petaurist to the Flying Squirrel, of the Wombat to the Beaver, of the Kangaroo to the Ruminant, of the Koala to the phytiphagous Sunbear, of the Opossums to the Shrews, and of the Echidna to the Anteater, &c., had been pointed out by myself. My esteemed friend and colleague Mr. Waterhouse, whilst admitting the justness of some of these comparisons, appended a timely warning, in a valuable note in his comprehensive and excellent history of the *Marsupialia*,† against the mistake to which the young zoologist might be liable, of concluding the analogical groups of the *Marsupialia* and *Placentalia* thus indicated to be of equal rank and value. I have always participated in this conviction of the lower value of the *Implacentalia* as compared with the *Placentalia;* and have used those terms merely as useful collective or general signs of certain modifications of structure, which are associated with the development and non-development of the placenta.

In like manner, when indicating the highest generalization to which I had arrived after comparisons of the dentition of the Mammalia by the terms 'monophyodont' and 'diphyodont,'‡ signifying respectively the single and double set of teeth developed in different groups of the class, I have been careful to guard myself from being misunderstood, as supposing that the monophyodont *Monotremata*, *Bruta*, and *Cetacea*, formed an equivalent group with the diphyodont bulk of the Mammalia, or that the binary groups, defined by this single dental character, were natural ones.

* Règne Animal, ed. 1829, vol. i, p. 174.

† Natural History of the Mammalia, 8vo, 1845, part i, p. 14. I must remark, however, that in stating "by Prof. Owen and some other naturalists, the present section (*Marsupiata*) is ranked as a subclass," the reader, from the peculiarly extended signification given to the term 'Marsupiata,' might be misled. The *Marsupialia* form one of the orders of my subclass *Implacentalia*. See the articles 'Marsupialia' and 'Monotremata,' in the "Cyclopædia of Anatomy," vol. iii, 1841.

‡ Cyclopædia of Anatomy, part xxxvii, 1849. Phil. Trans. 1850, p. 493.

Nothing more than a passing allusion seems needed to the system of classifying the Mammalia on the modifications of the placenta, originally proposed by Sir Everard Home,* and since reproduced and modified by a few other naturalists. The group, *e. g.* associated by the character of the discoid placenta, is as little natural as that which would be composed on the basis of the diphyodont dentition, or the unguiculate feet. The association of the *Rodentia*, and *Insectivora*, with the *Quadrumana*, as in the latest modification of the placentary system,† is not likely to command acceptance. The diffused placenta, as in the Mare, Porpoise, Peccari, Rhinoceros, and Camel, would lead to an equally heterogeneous assemblage. In two well-defined minor groups, *e. g.* the true *Carnivora* and the true *Ruminantia*, there exist characteristic modifications of the placenta, viz. the zonular and cotyledonal respectively; but though the zonular type is common to the *Carnivora*, it is not peculiar to them; it is that of the placenta in the Hyrax and the Elephant, amongst the *Ungulata*. So likewise the cotyledonal type characterizes the placenta of the Sloth among the *Bruta*.

(To be continued.)

ART. III.—*On the Rational Constitution of certain Organic Compounds;* by WOLCOTT GIBBS, M.D., Professor of Chemistry and Physics in the Free Academy in New York.

IN the following memoir I shall endeavor to establish the rational constitution of several important organic bodies by referring them to the type of one or more equivalents of water, in which hydrogen is wholly or partially replaced by compound radicals. The premises are as follows.

1. The electro-negative or chlorous hydrocarbons, formyl, C_2H, acetyl, C_4H_3, and their homologues, may replace hydrogen equivalent for equivalent, but by such replacement diminish the electro-positive or zincous character of the primitive. These facts are well exhibited in the acetyl-ammonia of Natanson, the formula of which is

$$N\left\{\begin{matrix}C_4H_3\\H\\H\end{matrix}\right\}.$$

2. The radicals formoxyl, C_2HO_2, acetoxyl, $C_4H_3O_2$, and their homologues and analogues, similarly replace hydrogen, and yield more electro-negative or chlorous derivatives. Thus acetamin has the formula

$$N\left\{\begin{matrix}C_4H_3O_2\\H\\H\end{matrix}\right\},$$

* Lectures on Comparative Anatomy, vol. iii, 4to, p. 445.

† GERVAIS, Zoologie et Paléontologie Française, 4to, 1853, p. 194.

while acetic acid is referable to the type of two equivalents of water, and has the formula

$$\left.\begin{matrix} C_4H_3O_2 \\ H \end{matrix}\right\} O_2.$$

3. One equivalent of *any* ammonium may replace one equivalent of hydrogen. Thus the formula of the hydrate of the oxyd of the primitive ammonium, NH_4O+HO, is also referable to the type of two equivalents of water, and we have for it the expression

$$\left.\begin{matrix} NH_4 \\ H \end{matrix}\right\} O_2.$$

4. Certain radicals, whether divisible or indivisible, are binary or ternary in character, and replace two or three equivalents of hydrogen. Thus the radicals C_2O_2, C_4O_4, and S_2O_4 are binary while nitrogen, phosphorus, &c. are ternary. These premises, as every chemist knows, are not new. I formulate them for convenience of reference, and to save repetition.

I propose now to apply these principles to certain bodies to which they have not hitherto been extended, and will consider the compounds in question *seriatim*.

Glycocoll or glycosin.—The empirical formula of glycosin was first definitely established by Horsford* in an elaborate memoir to which I shall frequently have occasion to refer. Of its rational constitution no satisfactory theory has been proposed. I refer it to the type of hydrate of oxyd of ammonium, and consider it to have the formula

$$N\left\{\begin{matrix} C_2HO_2 \\ C_2H \\ H \\ H \end{matrix}\right\} O+HO=C_4H_5NO_4.$$

This view of course (3) ultimately reduces glycosin to the type of two equivalents of water, $\left.\begin{matrix} H \\ H \end{matrix}\right\} O_2$. Glycosin, as is well known, combines with acids, bases, and salts. Its feebly acid character is explained, upon my view, by the presence of the two chlorous radicals; its basic properties, by the existence of two equivalents of hydrogen in the ammonium molecule; while its neutral character—which resembles that of water—may be explained by supposing that the chemical sum of the chlorous and zincous affinities in the ammonium approaches the character of an equivalent of hydrogen, so that we have the *functional* equation (*nearly true*),

$$N\left\{\begin{matrix} C_2HO_2 \\ C_2H \\ H \\ H \end{matrix}\right\} = H.$$

* Ann. der Chemie und Pharmacie, lx, 1.

The formulas of the principal salts of glycosin upon this theory are as follows.

Chlorhydrate, $N(C_2HO_2.C_2H.H_2)Cl+2HO.$

Nitrate, $N(C_2HO_2.C_2H.H_2)NO_4\}O_2+2HO.$

Sulphate, $N_2(C_2HO_2.C_2H.H_2)_2S_2O_4\}O_4+2HO.$

Potash-sulphate, $N_2(C_2HO_2.C_2H.HK)_2S_2O_4\}O_4.$ (*f*)

Potash-nitrate, $N(C_2HO_2.C_2H.HK)NO_4\}O_2+HO.$

Glycosin-lead, $N(C_2HO_2.C_2H.H.Pb.)H\}O_2+HO.$

Glycosin-copper, $N(C_2HO_2.C_2H.H.Cu)H\}O_2+HO.$

The platinum salt of glycosin contains, according to Horsford's analysis, 33·2 per cent of platinum, and he considers it to be

$$C_4H_4NO_2.PCl_2,$$

which requires that percentage. But Cahours* assigns to this salt the formula

$$C_4H_5NO_4.HCl+PCl_2,$$

which, upon the view which I have proposed, becomes

$$N\left\{\begin{matrix}C_2HO_2\\C_2H\\H\\H\end{matrix}\right\}Cl+PtCl_2+2HO.$$

The other salts of glycosin are easily formulated upon the ammonium theory, and do not require special notice. The combinations which glycosin forms with metallic oxyds are however of no small interest from the point of view suggested. Thus Boussingault found for the compound with oxyd of silver the formula

$$C_4H_4NO_2.AgO.$$

I believe that in this body silver simply replaces hydrogen, the true formula being

$$N\left\{\begin{matrix}C_2HO_2\\C_2H\\Ag\\H\end{matrix}\right\}O+HO,$$

and that the analogous bodies containing zinc, copper, lead, &c. have a similar constitution. This theory gives the simplest explanation of the formation and constitution of hippuric acid, and of the analogous acids which Cahours and other chemists have recently produced. Thus hippuric acid, as Dessaignes first showed, may be formed by the action of chlorid of benzoxyl upon glycosin-zinc, the reaction being explained by the equation

* Comptes Rendus, xliv, 569.

$$N\left\{\begin{matrix}C_2HO_2\\C_2H\\Zn\\H\end{matrix}\right\}O+HO+C_{14}H_5O_2.Cl=N\left\{\begin{matrix}C_2HO_2\\C_2H\\C_{14}H_5O_2\\H\end{matrix}\right\}O+HO+ZnCl.$$

Hippuric acid is consequently also the hydrate of an ammonium oxyd, a view which I shall develop more fully in another part of this paper. The cuminuric, anisuric and salicuric acids, which Cahours* has described, are formed in a similar manner, and must therefore have a similar constitution. By the action of the chlorids of acetoxyl $C_4H_3O_2.Cl$, butyroxyl $C_8H_7O_2.Cl$, &c., upon glycosin-zinc, similar acids must be produced, while it appears, to say the least, extremely probable, that the ethyl radicals, C_4H_5, &c. may also be made to replace an equivalent of hydrogen in glycosin, yielding bodies of an analogous acid and basic character. Thus by the action of iodid of ethyl upon glycosin-silver we should have

$$N\left\{\begin{matrix}C_2HO_2\\C_2H\\C_4H_5\\H\end{matrix}\right\}O+HO,$$

and it is of course possible that the last equivalent of hydrogen may be also replaced by an equivalent of a chlorous or zincous radical, as in the case of Hofmann's tetrammoniums.

The products of the decomposition of glycosin strongly support the view that this body contains the radicals formyl and formoxyl, as above assumed. Thus when fused with caustic potash, ammonia and hydrogen are evolved, while the fused mass contains cyanid of potassium and oxalate of potash. It is however well established that formic acid, $\left.\begin{matrix}C_2HO_2\\H\end{matrix}\right\}O_2$, under the same circumstances yields oxalic acid, and the facility with which formyl, C_2H, in contact with nitrogen or ammonia yields cyanogen, is also familiar to chemists With oxydizing agents glycosin yields carbonic and cyanhydric acids and water. This also is in exact accordance with the theory.

By the action of nitrous acid upon glycosin Socoloff and Strecker obtained a new acid which they termed glycolic acid, and which is homologous with lactic acid. The formula of this acid is $C_4H_4O_6$, and its formation is usually represented by the equation

$$C_4H_5NO_4+NO_3=2N+C_4H_4O_6+HO.$$

If we compare ammonia, NH_3, with three equivalents of water, we find that from the equation

$$NH_3=O_3H_3,$$

one equivalent of nitrogen may be considered as *formally* though not *functionally* replacing three equivalents of oxygen. Hence

* C. R., xliv, 567.

in the action of NO_3 upon oxyd of ammonium we may suppose that a species of *substitution* takes place, 3O replacing N, while, as in ordinary cases of substitution, a double molecule NN is separated. Thus the action of NO_3 upon oxyd of ammonium, considered for the sake of simplicity as anhydrous, may be represented by the equation

$$N\left\{\begin{matrix}H\\H\\H\\H\end{matrix}\right\}O+NO_3=O_3\left\{\begin{matrix}H\\H\\H\\H\end{matrix}\right\}O+2N=\left\{\begin{matrix}H\\H\\H\\H\end{matrix}\right\}O_4+2N.$$

When the oxyd of the ammonium is a monohydrate an equivalent of water is usually set free. This mode of considering the action of NO_3 upon ammonium molecules possesses advantages sufficient, as I think, to justify its employment. The formation of glycolic acid from glycosin may then be represented by the equation

$$N\left\{\begin{matrix}C_2HO_2\\C_2H\\H\\H\end{matrix}\right\}O+HO+NO_3=O_3\left\{\begin{matrix}C_2HO_2\\C_2H\\H\\H\end{matrix}\right\}O+2N+HO.$$

Glycolic acid has therefore the rational formula

$$\left.\begin{matrix}C_2HO_2\\C_2H\\H\\H\end{matrix}\right\}O_4,$$

and is referable to the type of four equivalents of water, though with the atomic weight here assigned to it, it contains but one equivalent of hydrogen replaceable by a metal. From this it appears that in the action of NO_3 upon an ammonium, the four radical molecules of the ammonium pass unchanged into the acid, and that an equivalent of water is separated as such when NO_3 acts upon the hydrate of an ammonium-oxyd. It must be observed however that, in most cases at least, it is only the last molecule of hydrogen which is replaceable in the new acid by a metal, this being the hydrogen molecule originally contained in the atom of water which united with the ammonia to form oxyd of ammonium, according to the equation

$$N\left\{\begin{matrix}X\\Y\\Z\end{matrix}\right.+HO=N\left\{\begin{matrix}X\\Y\\Z\\H\end{matrix}\right\}O.$$

Strecker has shown that the action of NO_3 upon hippuric acid produces a new acid which he terms benzoglycolic acid. The rational constitution of this acid must be represented by the formula

$$\left\{\begin{matrix}C_2HO_2\\C_2H\\C_{14}H_5O_2\\H\end{matrix}\right\}O_4=C_{18}H_8O_8,$$

and I shall endeavor to show, farther on, that there are other reasons for taking this view. Strecker's elaborate study of the bile has shown that cholic and hyocholic acid may each, like hippuric acid, be split into glycosin and a new acid. I apply to both these acids the theory which I have above developed, and consider them rationally represented as follows,

$$\text{Cholic acid,} \quad N\left\{\begin{matrix}C_2HO_2\\C_2H\\C_{48}H_{38}O_8\\H\end{matrix}\right\}O+HO=C_{52}H_{43}NO_{12}.$$

$$\text{Hyocholic acid,} \quad N\left\{\begin{matrix}C_2HO_2\\C_2H\\C_{50}H_{41}O_8\\H\end{matrix}\right\}O+HO=C_{54}H_{45}NO_{12}.$$

That there are homologous compound molecules having the formulas $C_{48}H_{39}O_8$ and $C_{50}H_{41}O_8$, is shown by the existence of cholalic and hyocholalic acids, the empirical formulas of which are $C_{48}H_{40}O_{10}$ and $C_{50}H_{42}O_{10}$, and which when reduced to the type of two equivalents of water become

$$\left.\begin{matrix}C_{48}H_{38}O_8\\H\end{matrix}\right\}O_2 \quad \text{and} \quad \left.\begin{matrix}C_{50}H_{41}O_8\\H\end{matrix}\right\}O_2.$$

I may here remark that by the action of NO_3 upon cholic and hyocholic acids we ought to obtain two new acids having the formulas

$$\left\{\begin{matrix}C_2HO_2\\C_2H\\C_{48}H_{38}O_8\\H\end{matrix}\right\}O_4=C_{52}H_{42}O_{14}, \text{ and } \left\{\begin{matrix}C_2HO_2\\C_2H\\C_{50}H_{41}O_8\\H\end{matrix}\right\}O_4=C_{54}H_{44}O_{14}.$$

I may further remark, in this connection, that we ought to be able to regenerate cholic and hyocholic acids from glycosin and cholalic and hyocholalic acids, by processes exactly analogous to that employed by Dessaignes in preparing hippuric acid. Thus cholalic acid by distillation with perchlorid of phosphorus should give chlorhydric acid and chlorid of cholalyl, according to the equation

$$C_{48}H_{40}O_{10}+PCl_5=C_{48}H_{39}O_9.Cl+PO_2Cl_3+HCl,$$

and chlorid of cholalyl with glycosin-silver should yield cholic acid and chlorid of silver. As cholalic and hyocholalic acids are homologous and have very high equivalents, it is almost certain that they are among the upper members of a complete series. Of this series chinovic acid, $C_{28}H_{30}O_{10}$, is probably a member.

Hippuric acid.—The formation of this acid by the action of chlorid of benzoxyl upon glycosin-zinc has already been explained upon the ammonium theory as a simple substitution of the radical $C_{14}H_5O_2$ for an equivalent of hydrogen. It remains to show upon the same theory that the separation of hippuric acid into glycosin and benzoic acid may be equally well ac-

counted for. The following equation appears to contain a simple solution of the question, since we have

$$N\left\{\begin{matrix}C_2HO_2\\C_2H\\C_{14}H_5O_2\\H\end{matrix}\right\}O+2HO=N\left\{\begin{matrix}C_2HO_2\\C_2H\\H\\H\end{matrix}\right\}O+C_{14}H_6O_4,$$

the glycosin-ammonium of course uniting at the moment of its formation with an equivalent of water. By this view of the constitution of hippuric and other similar acids, we explain at once its formation, its decomposition by boiling with acids, and its unibasic character, since its molecule contains only one equivalent of hydrogen replaceable by a metal. But the products of the decomposition of the acid by other agents also confirm the view here taken.

By boiling with peroxyd of lead, hippuric acid yields benzamid and carbonic acid, while with peroxyd of manganese and sulphuric acid, benzoic acid, carbonic acid, and sulphate of ammonia are formed. These reactions are expressed by the equations

$$N\left\{\begin{matrix}C_2HO_2\\C\ H\\C_{14}H_5O_2\\H\end{matrix}\right\}O+6O=N\left\{\begin{matrix}H\\H\\C_{14}H_5O_2\end{matrix}\right\}+2C_2O_4+HO,$$

and

$$N\left\{\begin{matrix}C_2HO_2\\C_2H\\C_{14}H_5O_2\\H\end{matrix}\right\}O+HO+6O+SO_3.HO=NH_4O.SO_3+C_{14}H_6O_2+4CO_2.$$

In both these cases the easily oxydized radicals, formyl and formoxyl, are destroyed, yielding carbonic acid and water, while in both the radical $C_{14}H_5O_2$ is found unchanged among the products of decomposition.

As glycosin by the action of NO_3 yields glycolic acid, so hippuric acid under the same circumstances yields benzoglycolic acid, thus we have for the derived acids

$$\text{Glycolic acid}\ \left\{\begin{matrix}C_2HO_2\\C_2H\\H\\H\end{matrix}\right\}O_4, \qquad \text{Benzoglycolic acid}\ \left\{\begin{matrix}C_2HO_2\\C_2H\\C_{14}H_5O_2\\H\end{matrix}\right\}O_4.$$

By boiling an aqueous solution of benzoglycolic acid, it is resolved into glycolic and benzoic acids, the reaction being

$$\left\{\begin{matrix}C_2HO_2\\C_2H\\C_{14}H_5O_2\\H\end{matrix}\right\}O_4+2HO=\left\{\begin{matrix}C_2HO_2\\C_2H\\H\\H\end{matrix}\right\}O_4+\left.\begin{matrix}C_{14}H_5O_2\\H\end{matrix}\right\}O_2.$$

Now this decomposition is precisely analogous to the resolution of hippuric acid into glycosin and benzoic acid, explained by the equation

$$\left\{\begin{matrix}C_2HO_2\\C_2H\\C_{14}H_5O_2\\H\end{matrix}\right\}NO+2HO=\left\{\begin{matrix}C_2HO_2\\C_2H\\H\\H\end{matrix}\right\}NO+\left.\begin{matrix}C_{14}H_5O_2\\H\end{matrix}\right\}O_2.$$

I bring forward these two parallel cases I believe for the first time. They are types of a great number of similar decompositions, and as I think, justify my two assumptions, first, that *one* equivalent of nitrogen *formally* replaces *three* equivalents of oxygen, or what is the same thing that $NO=O_4$, and second, *that for every ammonium-oxyd, or its hydrate, there is an acid or anhydrid corresponding to four equivalents of water, and in which the four radicals which replace the four equivalents of hydrogen are the same as the four in the original ammonium.* I believe that this principle will prove extremely fertile both in explanations and in new facts. I shall endeavor to develop this view more fully when speaking of other compound ammoniums.

In a note to the article Hippuric acid in the 3d volume of his Traité de Chimie Organique, p. 241, Gerhardt has suggested that hippuric acid, as well as glycosin, may be referred to the type of a hydruret of ammonium,

$$\left.\begin{matrix}NH_4\\H\end{matrix}\right\} = \left.\begin{matrix}H\\H\end{matrix}\right\}.$$

Upon this view, which is thrown out merely as a suggestion, Gerhardt represents glycosin as

$$\left.\begin{matrix}NH_2.(C_2HO_2)_2\\H\end{matrix}\right\}$$

and hippuric acid as

$$\left.\begin{matrix}NH.(C_2HO_2)_2C_{14}H_5O_2\\H\end{matrix}\right\}.$$

Strecker considers glycosin as possibly an ammonia having the formula

$$N\left\{\begin{matrix}C_4H_3O_4\\H\\H\end{matrix}\right\}$$

To this theory it may be objected that it does not explain the formation of alanin and other bodies of the same class, or the products of their decomposition.

Laurent looked upon glycosin either as amido-acetic acid—a view also recently taken by Cahours—or as the acid amid of glycolic acid. But Dessaignes has actually prepared glycolamid, and has shown that it is only isomeric and not identical with glycosin.

Alanin.—This body was obtained by Strecker by evaporating a mixture of aldehyd-ammonia and cyanhydric acid with chlorhydric acid, and its empirical formula is $C_6H_7NO_4$, so that it is homologous with glycosin. Its mode of formation appears to me to show in the clearest manner its molecular structure, and I consider it to have the rational formula

$$N\left\{\begin{matrix}C_4H_3O_2\\C_2H\\H\\H\end{matrix}\right\}O+HO.$$

If this view be correct we ought to find the radical $C_4H_3O_2$ among the products of the decomposition of alanin. In point of fact, when heated with peroxyd of lead it yields aldehyd, ammonia and carbonic acid, the carbon of the radical formyl C_2H alone being oxydized. With nitrous acid alanin yields lactic acid, the formula of which is thus found to be

$$\left\{\begin{matrix} C_4H_3O_2 \\ C_2H \\ H \\ H \end{matrix}\right\} O_4.$$

Now lactic acid, when heated with sulphuric acid and peroxyd of manganese, yields carbonic acid and an abundance of aldehyd, as Städeler has shown. Both the mode of formation and the products of decomposition of alanin support the view taken of its composition. But we may I think go farther. There are at least *three* bodies having the empirical formula $C_6H_7NO_4$; they are alanin, sarkosin, and lactamid. As sarkosin perfectly resembles alanin and glycosin in properties it must belong to the same series of bodies. I suggest that its rational formula may be

$$N\left\{\begin{matrix} C_2HO_2 \\ C_4H_3 \\ H \\ H \end{matrix}\right\} O + HO.$$

In this case it should give with nitrous acid a species of lactic acid isomeric with that derived from alanin and having the formula

$$\left\{\begin{matrix} C_2HO_2 \\ C_4H_3 \\ H \\ H \end{matrix}\right\} O_4.$$

There are however, as is well known, *two* lactic acids, the former derived from alanin by the action of nitrous acid, as well as from sugar by the lactic fermentation; while the other exists in the juice of flesh with sarkosin itself. It is proper to remark that one of the two equivalents of hydrogen in the above formulas appears, so far at least as our knowledge goes, not to be replaceable by a metal, so that lactic acid, like glycolic acid, is monobasic. On the other hand, as in the case of benzoglycolic acid, we have an acid in which this equivalent of hydrogen is replaced by an equivalent of benzoxyl. The primitive of this acid must be an acid hydrate of ammonium-oxyd homologous with hippuric acid. Thus we have, uniting the formulas in single lines to save space,

$$N\left\{\begin{matrix} C_4H_3O_2.C_2H_3.C_{14}H_5O_2.H \\ H \end{matrix}\right\} O_2 \qquad \text{Alanin-hippuric acid.}$$

$$C_4H_3O_2.C_2H.C_{14}H_5O_2.H\Big\} O_4 \qquad \text{Benzolactic acid.}$$

It is clear that alanin, sarcosin and their homologues will yield an indefinitely great number of new acids, of which one series will contain nitrogen, the other not.

The almost perfect analogy between alanin and glycosin justifies the inference that the internal molecular structure of these two bodies is the same. The formula of alanin which is assumed above is deduced from its mode of formation, and that of glycosin is based upon the assumption that this body may be formed in a similar manner, namely, by digesting together the aldehyd-ammonia of formic acid, $C_2HO_2.NH_3$, and cyanhydric acid. This theory cannot at present be put to the test of experiment because the formic aldehyd, $C_2HO_2.H$, has not yet been obtained. Another difficulty however arises. The formula of alanin assumed above supposes that formyl, C_2H, and acetoxyl, $C_4H_3O_2$, enter into the ammonium *as such*, each radical replacing an equivalent of hydrogen. It is however possible that these two radicals are in alanin so fused together as to constitute a single di-atomic radical, $C_6H_4O_2$, homologous with the glycolic radical, $C_4H_2O_2$. In this case the formulas of glycosin and alanin will be

$$\text{Glycosin,} \qquad N\left\{\begin{matrix} C_4H_2O_2 \\ H_2 \end{matrix}\right\}O+HO.$$

$$\text{Alanin,} \qquad N\left\{\begin{matrix} C_6H_4O_2 \\ H_2 \end{matrix}\right\}O+HO.$$

Upon this view the rational formulas of glycolic and lactic acids become

$$\left.\begin{matrix} C_4H_2O_2 \\ H_2 \end{matrix}\right\}O_4 \qquad \text{and} \qquad \left.\begin{matrix} C_6H_4O_2 \\ H_2 \end{matrix}\right\}O_4.$$

The decision of the question obviously turns upon the molecular structure of the radical $C_4H_2O_2$. Is this diatomic and indivisible, or is it a conjugate radical $C_2H.C_2HO_2$? On the other hand it appears possible that there may be two species of glycosin, of which the one contains formyl and formoxyl, the other the radical $C_4H_2O_2$. The rational formulas which I have assigned to glycosin and alanin appear, in the present state of our knowledge, most consistent with observed facts.

Leucin.—The formula of leucin upon my view becomes

$$N\left\{\begin{matrix} C_{10}H_9O_2 \\ C_2H \\ H \\ H \end{matrix}\right\}O+HO,$$

and the products of its decomposition bear out the theory completely. Limpricht* has prepared leucin from valeric aldehyd by a process exactly similar to that employed by Strecker in the formation of alanin. From this it follows that leucin is constituted, so far as the character of the radicals is concerned, like

* Ann. der Chemie und Pharmacie, xciv, 243.

alanin and not like sarcosin. It appears, however, probable that there exists another species of leucin having the formula,

$$N\left\{\begin{matrix}C_2HO_2\\C_{10}H_9\\H\\H\end{matrix}\right\}O+HO.$$

But the high equivalent of leucin points out to us the possible existence of three other bodies of a perfectly analogous structure and isomeric with ordinary leucin. Thus we have

$$N\left\{\begin{matrix}C_8H_5O_2\\C_9H_5\\H\\H\end{matrix}\right\}O+HO,\quad N\left\{\begin{matrix}C_8H_7O_2\\C_4H_3\\H\\H\end{matrix}\right\}O+HO,\quad \text{and } N\left\{\begin{matrix}C_4H_3O_2\\C_9H_7\\H\\H\end{matrix}\right\}O+HO.$$

Each one of these five species of leucin must yield a homologue of lactic acid, the only one yet discovered being the leucic acid of Strecker, which belongs to the series of the lactic acid of fermented sugar, and is derived from leucin by the action of nitrous acid.

It is easy to see that the homologues of glycosin, as we ascend in the series, for the same empirical constitution in the case of each one, must become more and numerous, and the same is consequently true for the homologues of glycolic acid.

Tyrosin.—The empirical formula of tyrosin, according to the analyses of Hinterberger, is $C_{18}H_{11}NO_6$. As the properties of this body are perfectly analogous to those of the members of the glycosin series, it may also be reduced to the same type, in which case its rational constition may perhaps be represented by the formula,

$$N\left\{\begin{matrix}C_{14}H_5O_2\\C_4H_3O_2\\H\\H\end{matrix}\right\}O+HO,$$

In nitrotyrosin we may suppose one of the two equivalents of hydrogen in the compound ammonium to be replaced by NO_4. The products of the decomposition of tyrosin have not however been studied sufficiently to enable us to assign its rational formula with any degree of certainty, and it is clear that the ammonium molecule assumed may be constituted in several different ways, so as to give the same empirical formula. Thus Wicke* suggests that tyrosin may be formed from anisic aldehyd and cyanhydric acid, as alanin is formed from acetic aldehyd. In this case the rational formula would be

$$N\left\{\begin{matrix}C_{16}H_9O_4\\C_2H\\H\end{matrix}\right\}O+HO,$$

it being remembered that $C_{16}H_9O_4$ has the value of two equivlents of hydrogen. Farther researches are required to settle this question.

* Ann. der Chemie und Pharmacie, ci, 314.

Anthranilic acid.—Kubel* has recently shown that anthranilic acid plays the part of a weak base with acids, yielding well defined salts with nitric, sulphuric and oxalic acids. The products of its decomposition by heat, viz., anilin and carbonic acid, are similar to those of leucin, which, according to Limpricht,† under the same circumstances yields amylamin and carbonic acid, the equations being

$$C_{12}H_{13}NO_4 = C_2O_4 + C_{10}H_{13}N,$$
$$C_{14}H_7NO_4 = C_2O_4 + C_{12}H_7N.$$

It is therefore probable that anthranilic acid belongs to the same type as leucin. Schwanert‡ adopts this view, and considering anthranilic acid as carbanilic acid, regards leucin as amyl-carbamic acid, a view which explains only one of the modes of decomposition of that body, and which is irreconcilable with all the rest. I suggest that anthranilic acid may contain the salicyl radical, $C_{14}H_4O_2$, and that its rational formula is

$$N\left\{\begin{matrix} C_{14}H_4O_2 \\ H \\ H \end{matrix}\right\} O + HO.$$

In the decomposition of anthranilic acid by heat, all the oxygen unites with two equivalents of the carbon of the salisoxyl, $C_{14}H_4O_2$, while all the hydrogen remains united with the other twelve equivalents of carbon and one of nitrogen to form anilin. The radical $C_{14}H_4O_2$ is of course to be regarded as diatomic. That anthranilic acid actually contains the radical $C_{14}H_4O_2$ appears most clearly from the fact that with nitrous acid it yields salicylic acid, by the process of replacement already explained. Thus we have

$$\left\{\begin{matrix} C_{14}H_4O_2 \\ H \\ H \end{matrix}\right\} NO + HO + NO_3 = \left\{\begin{matrix} C_{14}H_4O_2 \\ H \\ H \end{matrix}\right\} O_4 + 2N + HO.$$

Taurin.—This substance may be readily reduced to the type of hydrate of oxyd of ammonium, if we remark that the radical S_2O_4 replaces two equivalents of hydrogen. We have then for taurin the formula

$$N\left\{\begin{matrix} S_2O_4 \\ C_4H_5 \\ H \end{matrix}\right\} O + HO.$$

By fusion with caustic potash taurin yields acetate and sulphite of potash, a result which is easily explained upon the supposition that the ammonium molecule contains ethyl. By the action of nitrous acid taurin should yield an acid having the formula $C_4H_6S_2O_5$, since we should have the equation

$$N\left\{\begin{matrix} S_2O_4 \\ C_4H_5 \\ H \end{matrix}\right\} O + HO + NO_3 = \left\{\begin{matrix} S_2O_4 \\ C_4H_5 \\ H \end{matrix}\right\} O_4 + 2N + HO.$$

* Ann. der Chemie und Pharmacie, cii, 236.
† The same, ci, 295.
‡ Ann. der Chemie und Pharmacie, cii, 221.

The experiment has not to my knowledge been tried,* but Strecker's discovery that isethionate of ammonium by losing two equivalents of water is converted into taurin leaves little doubt that taurin is the ammonium of isethionic acid, and that the latter acid is to be referred to the type of four equivalents of water. Strecker's reaction is indicated by the rational equation

$$\left\{\begin{matrix}S_2O_4\\C_4H_5\\NH_4\end{matrix}\right\}O_4-2HO=\left\{\begin{matrix}S_2O_4\\C_4H_5\\H\end{matrix}\right\}NO+HO.$$

Choleic acid may be regarded as taurin in which an equivalent of hydrogen in the ammonium is replaced by an equivalent of the radical of cholalic acid. Its formula upon this view is

$$N\left\{\begin{matrix}S_2O_4\\C_4H_5\\C_{48}H_{39}O_8\end{matrix}\right\}O+HO,$$

It must be remarked however that this formula contains one equivalent of water less than that deduced by Strecker from his analyses. Assuming it to be correct, it shows that there exists the same relation between taurin and choleic acid as between glycosin and cholic acid. It would be interesting, from a physiological as well as from a chemical point of view, to determine by experiment whether cholalic acid taken into the stomach would be found in the urine in the form of cholic acid, a result which might be expected from well known facts in relation to the conversion of benzoic into hippuric acid under the same circumstances. Should this result be obtained it would show that the kidneys are capable of producing under certain circumstances a true biliary secretion.

Asparagin.—The formula of this body may be reduced to the type of two equivalents of oxyd of ammonium, but the data for determining the particular character of the radicals replacing hydrogen are not at present sufficient. I shall therefore content myself with suggesting that asparagin may have the formula

$$\left.\begin{matrix}N\left\{\begin{matrix}C_2HO_2.C_2H.H_2\\C_2HO_2.C_2H.H_2\end{matrix}\right\}\\N\end{matrix}\right\}O_2,$$

in which case its relations to glycosin are obvious. It may also be referred to the same type by supposing that it contains glyoxal, the formula being then

$$\left.\begin{matrix}N\left\{\begin{matrix}C_4H_3O_3.H_2\\C_4H_3O_3.H_2\end{matrix}\right\}\\N\end{matrix}\right\}O_2.$$

These views will be discussed more fully in speaking of malic acid.

* Since the above was written, I have made the experiment in question and with complete success, obtaining isethionate of potash by the action of nitrite of potash upon taurin dissolved in dilute nitric acid.—W. G.

In concluding the first section of this paper, which embraces only nitrogenous compounds performing at the same time the functions of acids and bases, I shall venture to suggest that the nitrogenous animal and vegetable substances, known as proteine bodies, belong to the same class as glycosin, leucin, &c. As the equivalents of these bodies are high, and as they contain more than one equivalent of nitrogen, they may perhaps be referred to the type of two or four equivalents of oxyd of ammonium or its hydrate. Another view which may be taken is that they contain, as substitutes for hydrogen, amids of the type $N\left\{\begin{matrix}H\\H\end{matrix}\right\}$, each such amid replacing a single equivalent of hydrogen. In this manner they may possibly be reduced to the type of one equivalent of oxyd of ammonium. Thus a body containing four equivalents of nitrogen may be represented by the formula

$$N\left\{\begin{matrix}NX_2\\NY_2\\NZ_2\\V\end{matrix}\right\}O+HO.$$

It is well known that the so-called proteine bodies are generally soluble both in acids and alkalies. By the action of oxydizing agents they yield, as first products of their decomposition, various nitriles, valeronitril, benzonitril, &c., in this respect exactly resembling glycosin, leucin, &c. Setting out from this view I have examined the action of nitrous acid upon gelatin, casein, and albumin, and have found that this agent exerts a more or less powerful action upon each of these bodies. A solution of gelatin, for example, is decomposed by a current of NO_3 with strong effervescence, while an acid liquid remains, the examination of which is not yet complete. In the present imperfect state of our knowledge of the products of the oxydation of gelatin and the proteine bodies, it appears almost hopeless to attempt to represent them by formulas, even if we neglect the small quantities of sulphur and phosphorus which they always contain. To illustrate my suggestion as to their constitution, however, I will give a formula for albumen which will represent tolerably well the products of its decomposition and serve to exhibit the type to which it perhaps belongs. The analyses of albumen of white of eggs are nearly represented by the empirical formula*

$$C_{72}H_{24}N_4O_{12}.$$

* Scheerer found

		Theory.
Carbon,	54·3	54·5
Hydrogen,	7·1	6·8
Nitrogen,	15·7	15·9
Oxygen,	—	22·7
		100·00

Upon the view above taken that albumen is reducible to the type of hydrate of oxyd of ammonium in which hydrogen is partially replaced by certain amids, we may assign to it the formula

$$N\left\{\begin{matrix} N.H.C_{14}H_5O_2 \\ N.H.C_{12}H_{11}O_2 \\ N.H.C_4H_3O_2 \\ C_2HO_2 \end{matrix}\right\} O+HO=C_{32}H_{24}N_4O_{10}.$$

I again observe that I bring forward this formula simply as an illustration of a particular view, and not as a definite expression for the constitution of albumen. Speculations of this kind are not without value, since it is, to say the least, possible that all the *crystalline* nitrogenous vegetable and animal products are reducible to the same general type of two or more equivalents of water in which hydrogen is more or less completely replaced by complex ammonium molecules. I will even venture to hazard the suggestion that the entire vegetable or animal organism may be reduced to the water type, since resins, essential oils, and other bodies belonging to the type of hydrogen, are either secretions or excretions, and do not form a part of the organism itself. One of the problems of physiology must then be to reduce all parts of the organism to the least possible number of water types, and to show how each constituent may be derived from water by perfectly definite and uniform processes of substitution.

In considering the action of nitrous acid upon the ammoniums or their hydrates, I have endeavored to show that the action may be regarded as a regular process of substitution in which one equivalent of nitrogen replaces three equivalents of oxygen. The cases which I have mentioned are by no means the only ones which illustrate this species of replacement, since it may be used to explain the action of ammonia upon a great number of organic compounds. Thus hydrobenzamid results from the action of ammonia upon oil of bitter almonds according to the equation,

$$3C_{14}H_6O_2+2NH_3=C_{42}H_{18}N_2+6HO.$$

We may here suppose O_6 to be simply replaced by N_2, since we have a binary substituting molecule, namely, $N_2|H_6$, acting upon the six equivalents of oxygen, one-half of the binary molecule taking the place of the oxygen displaced. If we admit that oxygen and nitrogen replace each other in the manner supposed, we may reduce a great number of nitrogenous compounds directly to the same type without the intervention of any *special* theory whatever. Thus $C_{42}H_{18}N_2$ and $C_{42}H_{18}O_6$ would belong to the same type. Substitutions of oxygen for nitrogen sometimes occur, as in the conversion of acetonitril into acetic acid, since we have

$$C_4H_3N+3HO=C_4H_3O_3+NH_3.$$

§ 2.

The views above developed have led me to the consideration of the rational constitution of certain organic acids, some of which have been already mentioned. In endeavoring to establish the formulas of these acids I set out from the principle that the type of the acid is to be deduced from its mode of formation, and from the products of its decomposition, and not merely from the number of equivalents of replaceable hydrogen which it contains. Thus the empirical formula of lactic acid is $C_6H_6O_6$, and as the acid with this formula is monobasic, it is usually reduced to the type of two equivalents of water,

$$\left.\begin{matrix} C_6H_5O_4 \\ H \end{matrix}\right\} O_2,$$

the compound $C_6H_5O_4$ being considered as having the value of one equivalent of hydrogen.

I have endeavored to show that, inasmuch as lactic acid is derived from alanin by the replacement of O_2 by N, and separation of an equivalent of water, the true type is that of *four* equivalents of water, the rational formula being

$$\left.\begin{matrix} C_4H_3O_2 \\ C_2H \\ H \\ H \end{matrix}\right\} O_4,$$

and upon this view I have endeavored to explain the difference between the lactic acid derived from flesh and that formed in the fermentation of sugar, this difference being explained not by the assumption of two isomeric radicals, $C_6H_5O_4$, but by an actual difference in the structure of the acid. I have suggested also that there may be a species of glycosin having the formula

$$N\left\{\begin{matrix} C_6H_2O_2 \\ H_2 \end{matrix}\right\} O+HO.$$

In this case there must also be a body isomeric with alanin, and having the formula

$$N\left\{\begin{matrix} C_6H_4O_2 \\ H_2 \end{matrix}\right\} O+HO,$$

supposing of course that the two radicals which I have assumed in alanin, namely, $C_4H_3O_2$ and C_2H, are not fused together to form $C_6H_4O_2$, but exist separately. Such a body would give a lactic acid having the rational formula

$$\left.\begin{matrix} C_6H_4O_2 \\ H_2 \end{matrix}\right\} O+HO.$$

In what precedes I have assumed that lactic acid is monobasic, while it is more usual to double its formula and consider it as bibasic. It must however be remembered that, according to Strecker's observations, the density of the vapor of lactate of ethyl corresponds to four volumes only upon the supposition that the acid is monobasic. The rational formula above pro-

posed contains *two* equivalents of free hydrogen, and it would therefore appear that lactic acid, even upon my view, should be bibasic. I have already stated that—as an empirical result—when an acid belonging to the type of *four* equivalents of water is derived from an ammonium or its hydrated oxyd, it is only the last equivalent of hydrogen which is replaceable by another radical to form a salt. There must be a reason for this, and perhaps either or both of the following may be satisfactory.

In the first place it appears probable that in all the salts of ammonium the *fourth* equivalent of hydrogen is differently combined from the other three, and if this view be correct it is reasonable to suppose that this peculiarity will exist also in the acids which are derived from ammoniums. Again it may be that in glycosin, alanin, &c., and therefore in the acids derived from them, the two molecules constituting the aldehyds from which these bodies are derived, enter *in connection* and not separately, as I have all along represented them in the present paper. Thus alanin may be

$$N\left\{\begin{matrix}C_4H_3O_2.H\\C_2H\\H\end{matrix}\right\}O+HO.$$

In which case the lactic acid derived from it will be

$$\left.\begin{matrix}C_4H_3O_2.H\\C_2H\\H\end{matrix}\right\}O_4.$$

and it is easy to see from this formula that there will be but one equivalent of replaceable hydrogen in the acid. This latter view is perhaps supported by the mode of formation of acetonic acid which Städeler obtained by digesting acetone with cyanhydric and chlorhydric acids, and which has the empirical formula $C_8H_8O_6$. Its rational formula upon the above view will be

$$\left.\begin{matrix}C_6H_3O_2.C_2H_3\\C_2H\\H\end{matrix}\right\}O_4.$$

My reason for not adopting in the outset the slight modification of the rational formulas just proposed is to be found in the fact that in glycosin, alanin and their congeners one equivalent of hydrogen—the last but one—is replaceable by an equivalent of silver or another metal. If now we suppose that these bodies contain aldehyds, we must admit that it is in each case the hydrogen molecule of the aldehyd which is replaced by the metal, or that there are compounds like $C_4H_3O_2.Ag$, which is not supported by any experimental evidence. The point is after all of but little importance, and does not affect the theory in any essential particular. The so-called anhydrous lactic acid which has the empirical formula $C_6H_5O_5$ may be reduced—after doubling its equivalent—to the type of *six* equivalents of water, and will then have the rational formula

$$\left.\begin{matrix} C_4H_2O_2.H \\ C_2H \\ C_4H_2O_2.H \\ C_2H \end{matrix}\right\} O_6.$$

Lactid, $C_6H_4O_4$, may be reduced to the type of two equivalents of water, and is rationally

$$\left.\begin{matrix} C_4H_2O_2 \\ C_2H \end{matrix}\right\} O_2.$$

Lactamid which is isomeric with alanin appears to belong to the type of two equivalents of water since it yields lactic acid and ammonia by taking up two equivalents of water. It is however difficult in the present state of our knowledge to assign a rational formula for bodies of this class. The same remark applies to lactamic acid.

Malic acid.—Some idea of the rational constitution of malic acid may be obtained from an attentive consideration of the products of its decomposition. Caustic potash at a high temperature converts the acid into oxalic and acetic acids, two equivalents of oxygen being required for the oxydation, since we have the equation

$$C_3H_6O_{10} + 2O = C_4H_4O_4 + C_4H_2O_8.$$

Bromine decomposes the acid with formation of bromoform. According to Vauquelin, nitric acid converts it into oxalic acid. Sulphuric acid gently heated with the acid yields carbonic oxyd and, according to Liebig, acetic acid. Finally, a mixture of sulphuric acid and bichromate of potash converts all the carbon into carbonic acid. I refer malic acid to the type of six equivalents of water, and consider it to contain either formyl and formoxyl, or glyoxal and formyl, so that its rational formula will be either

$$(1.)\quad \left.\begin{matrix} C_4H_2O_4 \\ C_2H \\ C_2H \\ H_2 \end{matrix}\right\} O_6, \quad \text{or} \quad (2.)\quad \left.\begin{matrix} C_2HO_2 \\ C_2HO_2 \\ C_2H \\ C_2H \\ H_2 \end{matrix}\right\} O_6.$$

If we admit that two equivalents of formoxyl, C_2HO_2, may be so fused together as to form one equivalent of glyoxal, we may also suppose that two of formyl, C_2H, may unite to form one equivalent of a diatomic radical having the formula C_4H_2. In this case the rational formula of malic acid, still referred to the type of six equivalents of water, will be

$$(3.)\quad \left.\begin{matrix} C_4H_2O_4 \\ C_4H_2 \\ H_2 \end{matrix}\right\} O_6.$$

In his memoir on glyoxal and its derivatives, Debus has suggested that a relationship between glycolic, malic, citric, glyoxylic and tartaric acids, may be traced by means of glyoxal, without however attempting in this way to express the rational

constitution of these acids, or in fact anything more than their formation and modes of decomposition. Representing glyoxal $C_4H_2O_4$ by the symbol gly, we have for the acids in question the following scheme.

Glycolic acid,	gly.H_2O_2.	Glyoxylic acid,	gly.O_2.
Malic acid,	gly 2.H_2O_2.	Anhyd. tartaric acid,	gly 2.O_2.
Citric acid,	gly 3.H_2O_2.		

Debus shows further how citric, malic and glycolic acids by losing two equivalents of water form aconitic and maleic acids, and glyoxal. I am disposed to go much farther and represent the rational constitution of a number of organic acids by supposing them to contain glyoxal or diformoxyl, and formyl or diformyl. It appears to me that it may reasonably be doubted whether the glycolic acid obtained by the action of nitrous acid upon glycosin is identical with that which Debus obtained by boiling glyoxal with milk of lime. I mean of course here to assume that ordinary glycosin may be formed by digesting formic aldehyd, $C_2HO_2.H$, with cyanhydric and chlorhydric acids, so that its structure is similar to that of alanin. There may be more than one species of glycosin, one being for instance

$$\left.\begin{matrix} N.C_2HO_2.C_2H.H_2 \\ H \end{matrix}\right\} O_2,$$

while another is

$$\left.\begin{matrix} N.C_4H_2O_2.H_2 \\ H \end{matrix}\right\} O_2.$$

But bearing these distinctions in mind, I shall endeavor to give rational formulas for certain organic acids involving as few assumptions as possible, and connecting those which are not homologous. The formula (1.) which I have given for malic acid explains sufficiently well the products of its decomposition, since glyoxal by taking up two equivalents of hydrogen becomes acetic acid, while the oxydation of $C_4H_2O_4$ or of C_4H_2 will account for the formation of oxalic and carbonic acids. Maleic and fumaric acids have the formula $C_8H_6O_8$. If we suppose that the action of heat simply splits the equivalent of glyoxal in malic acid into two equivalents of formoxyl, C_2HO_2, and that one eq. of formoxyl loses two eq. of oxygen, the formula common to the two pyrogenic acids becomes

$$\left.\begin{matrix} C_2HO_2 \\ C_2H \\ C_2H \\ C_2H \\ H_2 \end{matrix}\right\} O_6 = C_8H_6O_8.$$

This formula gives no explanation of the difference between the two acids. It may be that, in one of these, two molecules of formyl, C_2H, are united to form a molecule of diformyl, C_4H_2, but further researches are necessary before any satisfactory explanation can be proposed.

The formulas which I have given for malic acid lead to the conclusion that asparagin, $C_8H_8N_2O_6$, is referable to the type of two equivalents of oxyd of ammonium, and it may have the rational formula

$$N_2 \left\{ \begin{matrix} C_4H_2O_4 \\ C_2H \\ C_2H \\ H_4 \end{matrix} \right\} O_2.$$

By the action of nitrous acid asparagin becomes

$$\left. \begin{matrix} C_4H_2O_4 \\ C_2H \\ C_2H \\ H_4 \end{matrix} \right\} O_3,$$

which immediately splits into one equivalent of malic acid and two of water. Aspartic acid may also be reduced to the type of six equivalents of water, if, as is certainly admissible, we suppose NH_2 to replace H, so that we have as the rational formula

$$\left. \begin{matrix} C_4H_2O_4 \\ C_2H \\ C_2H \\ NH_2 \\ H \end{matrix} \right\} O_5.$$

On the other hand this view does not account for the basic properties of aspartic acid which, as is well known, forms definite compounds with the stronger acids. We meet here with the same difficulty which occurs in formulating benzamic acid and many other similar bodies.

Tartaric acid.—The researches of Dessaignes have shown that glyoxal is a product of the oxydation of tartaric by nitric acid, and the facility with which oxydizing agents convert tartaric acid into formic and carbonic acids has long been known. By fusion with caustic potash tartaric acid yields acetic, oxalic, formic and carbonic acids as I have myself observed, while at the same time an inflammable gas burning with a yellow flame is given off: this gas appears to be only an impure hydrogen. If we regard tartaric acid as bibasic I suggest that its rational formula may be

$$\left. \begin{matrix} C_4H_2O_4 \\ C_2HO_2 \\ C_2H \\ H_2 \end{matrix} \right\} O_5,$$

which places its relation to malic acid in the clearest light, and at the same time accounts for the facility with which it is decomposed, and for the products of its decomposition. Our knowledge of the modes of decomposition of pyrouvic and pyrotartaric acids is not sufficiently complete to permit us to form any idea of their rational constitution.

Citric acid.—The combinations of citric acid with ethyl and methyl conclusively show that this acid is tribasic and that its

formula is $C_{12}H_8O_{14}$. Those chemists who have endeavored to reduce it to the type of water refer it to six equivalents and represent it by the formula

$$\left.\begin{matrix} C_{12}H_8O_8 \\ H_6 \end{matrix}\right\} O_6.$$

I suggest that it may be referred to the type of eight equivalents of water and that its rational formula is

$$\left.\begin{matrix} C_4H_3O_2 \\ C_4O_4 \\ C_2H \\ C_2H \\ H_2 \end{matrix}\right\} O_8,$$

so that it contains the radicals of acetic and oxalic acids together with two equivalents of formyl. The view here taken of the rational constitution of citric acid appears to be supported by the character of the products of its oxydation. Thus, according to Gay-Lussac, citric acid fused with caustic potash yields a mixture of acetate and oxalate of potash. Concentrated sulphuric acid disengages pure carbonic oxyd almost without the aid of heat. Peroxyd of manganese and sulphuric acid transform citric into formic and carbonic acids. Finally, concentrated nitric acid oxydizes citric acid to a mixture of oxalic and acetic acids, while carbonic acid is evolved. All these reactions are easily and simply explained upon the hypothesis that citric acid contains within its molecule the radicals above mentioned. The products of the decomposition of the acids derived from citric acid by heat have not yet been studied, and it is therefore useless to endeavor to assign to these acids rational formulas. It seems moreover probable that there is no connection between the radicals in the primitive acid and in its pyrogenic derivatives, since the action of heat may be reasonably supposed to break up the radicals of the primitives and unite their constituents in entirely different proportions. The same remark applies to powerful agents, like chlorine and bromine, which generate at the same time products of oxydation and of substitution.

Many other organic acids may be formulated upon the principles which I have here laid down, and I venture to think that these principles will hereafter be found of extensive application. I have endeavored, and I believe with success, to reduce the formulas of the class of bodies known as glucosids to the types of multiples of water, but the recent publication of special researches of Berthelot has anticipated the principal results which I had obtained. I conclude with the expression of my conviction that every complex organic molecule is built up, not directly of the elements which it contains, but of simpler organic molecules, which are more or less perfectly fused together but which may yet in the majority be distinctly traced in the complex whole.

New York, Oct. 27th, 1857.

ART. IV.—*The Estimation of the Weights of very small portions of Matter;* by ALFRED MCMAYER, Professor of Physics and Chemistry in the University of Maryland.

THE chemist, in the course of his analytical investigations, often meets with what are called traces of substances; by which is generally understood, quantities of matter too minute to have any appreciable weight in the analytical balance. Now it sometimes happens that these traces are of as much importance considered scientifically and commercially as the ingredients present in appreciable quantities; and in order to estimate these small portions of matter he is often obliged to go over his work, using very considerable weights of substances, whereby his time and care are nearly doubled. It was this inconvenience that first induced me to try to determine in one operation the components present in large and in very minute quantities; and although I have succeeded beyond my expectations, I am confident that the process is susceptible of improvement, both as regards sensibility and accuracy.

After making many investigations on the sensibility of the most delicate levers as to small weights, this method was found far too rough. It then occurred to me that if instead of using the opposing force of gravity through the intervention of a lever, we could oppose to the gravitating effect of the matter the force of perfect elasticity as manifested in filaments of glass, we might succeed in obtaining the weights of extremely small parts of matter. For that purpose I tried the elasticities both of torsion and flexure, and found the latter only to answer the purpose.

The following is a description of the construction of my apparatus with which I have succeeded in estimating portions of matter equal in weight to the thousandth part of a milligram. Heating a rod of soft glass in one spot to bright redness, I drew it out quickly, and thereby obtained a filament uniformly cylindrical, of about the diameter of fine human hair. Taking from the middle of this fine glass thread a piece of such a length (about three inches) that its weight would barely reduce it from the horizontal, one end of it was fastened by means of good sealing wax to the edge of a mahogany block, and the other end slightly hooked by approaching quickly a small spirit flame. In order to obtain a pan in which to place the substance whose weight I would estimate, I cut with the common microscopic section-cutter some disks of elder pith from ·001 to ·002 inch in thickness; and drawing out a still finer filament, the end was likewise hooked, and the other extremity being passed through a pith disk, a small knob of glass was made on this end by the spirit flame, just of sufficient size to prevent this disk slipping

off the suspending rod. The filament with attached disk was now hooked on the end of the rod fixed to the block, and was then ready for graduation.

Not being able at the time to procure silver wire of sufficient fineness, I substituted some very fine and long hair, taken from the head of a child; and having brought the centre of gravity and centre of motion of a very sensitive analytical balance almost to coincide, I obtained a piece of the middle of a hair weighing exactly one-half milligram. This being divided into five equal parts (each about one inch long) gave us tenths of a milligram. One of these tenths being placed on the pith-pan, the glass filament was deflected a certain quantity, which was marked on an arc formed of bristol board, and so as to be almost touched by the deflected rod in its revolution about the edge of the block. Another tenth was added and another division obtained: and so on, until all five divisions were marked. The length of the divisions being about one-fourth of an inch, they were very readily subdivided into ten equal parts which gave me immediately $\frac{1}{100}$ths of a milligram. The weight of any quantity of matter less than one-half milligram may be now estimated to $\frac{1}{100}$th of a milligram by placing it on the pan and observing the deflection.

For the thousandths, still more care and patience is required, the filament being much finer and somewhat shorter, and the pith disk smaller and as thin as possible. In order to obtain the primary graduations of hundredths, one of the above pieces of hair equal to $\frac{1}{10}$th milligram is divided into ten equal parts, which gives us weights of $\frac{1}{100}$th milligram. The deflections caused by these weights, divided into ten equal parts, give $\frac{1}{1000}$ of a milligram.

As the least breath of air interferes with the graduations and weighing, the whole instrument is protected by a glass case, the end of the case next the graduated arc being on a hinge.

In elastic rods of square section, the deflection is proportional to the weight; in those of circular section this law is slightly departed from; but by the above method of ascertaining directly the value of each division, the error is avoided.

Those who may have the necessity to construct this apparatus should arm themselves beforehand with scrupulous care and unbounded patience.

From the great simplicity of the above arrangement, it seems very strange that some person did not long ago invent it; but, to my knowledge, it has never been attempted.

Baltimore, Md., Oct. 26th, 1857.

ART. V.—*On the Behavior of the Carbonates of Lime and of Baryta in presence of various Saline Solutions. With remarks on the Determination of Carbonic Acid in Mineral Waters;* by FRANK H. STORER.

THE object of the following article is to call attention to the very general influence which aqueous solutions of the alkaline salts, including those of ammonia, exert in preventing the precipitation of the carbonates of lime and of baryta. It will moreover be shown that solutions of the hydrates of baryta and of lime when diluted with water, or with dilute solutions of the caustic alkalies, are no longer precipitated by carbonic acid gas. For convenience the latter subject will be first treated of.

A current of carbonic acid gas passed through baryta water, diluted with two or three times its volume of water, until it was completely saturated, afforded no precipitate at any moment during the process, nor was any produced when the excess of carbonic acid was driven off by long continued ebullition. The solution even regained its strong alkaline reaction and a precipitate was at once formed in it, on addition of a solution of an alkaline carbonate. If a dilute solution of caustic soda, potash, ammonia or of lime be added to the baryta water, from which the excess of carbonic acid has been driven by boiling, and the solution again boiled, a precipitate of carbonate of baryta is produced; this precipitate does not form, however, if a sufficiently dilute solution of the caustic alkali has been used, unless the solution be heated, while that formed on addition of a solution of an alkaline carbonate falls even in the cold. There is also a point where less dilute solutions of the caustic alkalies produce precipitates in the cold when added to a solution which has afforded no precipitate on boiling.

It should be mentioned that the foregoing refers to a solution of hydrate of baryta of constant strength, since at different degrees of concentration very different reactions may occur. Thus, in concentrated solutions a current of carbonic acid gas produces a precipitate at once; in a solution slightly diluted it produces a precipitate only when heated; in a solution still more diluted no precipitate is produced even on continued ebullition.

From these experiments the two following conclusions may be drawn. (1.) That baryta water when diluted loses, in great part, its power of absorbing carbonic acid gas. (2.) That a small portion of carbonic acid remains, in solution, in the baryta water even in spite of long continued ebullition.

When diluted with several times its volume of water, lime water also ceases to afford an immediate precipitate when carbonic acid gas is passed through it. A precipitate generally

forms on boiling, but if the lime water be much diluted no precipitate will occur even on actual ebullition, although solutions of the alkaline carbonates produce at once precipitates.

If to the lime water in which carbonic acid gas has produced no precipitate even after boiling, a dilute solution of caustic soda, ammonia or lime be added and the mixture heated, a precipitate of carbonate of lime will be formed. This shows that a small portion of carbonic acid is retained by the lime water even after long continued ebullition as it was by baryta water. Even saturated lime water is capable of retaining a portion of carbonic acid in solution for a considerable time if the solution is not heated. This is easily proved by passing carbonic acid into lime water until it is partially saturated, when on filtering and boiling the clear filtrate an abundant precipitate of carbonate of lime is formed. This precipitate dissolves with effervescence in dilute acids and presents the characteristic needles of aragonite when examined under the microscope. I have noticed that lime water which has been exposed to the air, as when kept in bottles with loosely fitting stoppers, affords an abundant precipitate of carbonate of lime on boiling, but if that which has been thus exposed be afterwards placed in a securely closed bottle it will deposit after a few days all the carbonate of lime which it had previously retained in solution and will no longer afford any precipitate of it, on boiling. The well known incrustation of hydrate of lime is always deposited when lime water is boiled, but this for the most part becomes firmly fixed to the sides of the vessel, presenting, even to the naked eye, a very dissimilar appearance from the precipitate of carbonate of lime and on examination with the microscope the crystals of hydrate of lime have the appearance of rectangular plates, while those of carbonate of lime (produced by ebullition) are acicular.

These remarkable reactions, namely, the nonformation of a precipitate when carbonic acid gas is passed into dilute baryta or lime water, must, I think, depend upon the affinity which the great mass of water present exerts for the baryta or lime, or, upon the inertia of the dissolved alkaline earth, in other words, its indisposition to separate from the water. Moreover, since the carbonates of the alkaline earths are somewhat soluble in water their tendency to form a precipitate must be, under the circumstances, comparatively slight. Baryta or lime and carbonic acid are thus capable of existing simultaneously in solutions in a quantity of water which would not redissolve a fraction of the amount of carbonate of baryta or lime which they would produce if once precipitated.

The actions of carbonic acid gas on solutions of caustic lime or baryta when mixed with dilute solutions of the caustic alkalies may be mentioned here as it seems to be connected with that where water alone is present.

Lime water mixed with a *dilute* solution of caustic soda, potash or ammonia gives no immediate precipitate when carbonic acid gas is passed into it, unless the solution is boiled. Baryta water yields analogous results. If with the latter, instead of the solution of soda, an equal bulk of water is used there will be no precipitate even on boiling. This shows that when water alone is present, the carbonic acid, at first dissolved, is driven off on the application of heat before it can react on the hydrate of baryta, while if soda is used this alkali retains the carbonic acid which, on heating, reacts upon the baryta.

I pass next to consider the influence which aqueous solutions of the alkaline salts exert in preventing the precipitation of the carbonates of lime and of baryta.

That solutions of many of the ammoniacal salts exert a solvent power upon the carbonates of lime and of baryta, especially when recently precipitated, has been noticed by several observers (A. Vogel, *J. pr. Chem.*, vii, 453; Wittstein, *Report. f. Pharm.*, lvii, 18; Wackenroder, *Ann. Pharm.*, xli, 315; Brett, *London and Edin. Phil. Mag. and Journal of Science*, x, 95). Solutions of the salts of potash have also been noticed to possess the same power, though to a less degree, and I find that the salts of soda stand midway in this respect between those of ammonia and of potash, and that even chlorid of calcium exerts a certain solvent power upon recently precipitated carbonate of lime.

This solvent action may be seen by treating the recently precipitated carbonate with a great excess of a solution of almost any alkaline salt, but is observed more distinctly in the great tendency of the alkaline salts to prevent the precipitation of the carbonates of the alkaline earths. Thus, if lime water be mixed with an aqueous solution of chlorid of ammonium, chlorid of sodium or chlorid of potassium, and a current of carbonic acid gas be passed into the mixture no precipitate is produced, even on boiling, if the alkaline chlorid be present in sufficient quantity. If less of the alkaline chlorid has been used there will be a precipitate formed on boiling although none has occurred in the cold. Chlorid of calcium exerts an action entirely analogous to that of the alkaline chlorids, though, so far as I have observed, a precipitate always forms on boiling.

A solution of sulphate of ammonia or of sulphate of soda, when mixed with lime water exerts an influence almost precisely like that of the alkaline chlorids, carbonate of lime not being precipitated even on boiling if they are present in sufficient quantity.

A solution of sulphate or of nitrate of potash behaves much like that of sulphate of soda, but its influence is less strongly marked.

From the extreme facility with which water alone prevents the precipitation of baryta water by carbonic acid, it is difficult to judge of the influence which salts in solution in it may exert. Chlorid of ammonium, however, retards in a marked manner the precipitation of carbonate of baryta from baryta water, a portion remaining in solution even after boiling with carbonate of soda. Chlorid of sodium and of potassium also retard in a measure the precipitation of carbonate of baryta, but their action is not well marked.

The solvent influence of the alkaline salts can be observed with equal facility by mixing their solution with that of an alkaline carbonate and adding a solution of a lime or baryta salt to the mixture. A few examples will illustrate this point.

(1.) A solution of chlorid of calcium produces no precipitate, except on boiling, when added to a mixed solution of carbonate of soda and sulphate of soda excepting when the carbonate is in excess. In this experiment the sulphate of soda may be replaced by any of the alkaline sulphates or chlorids.

(2.) A solution of chlorid of barium produces no precipitate, except on boiling, when added to a mixed solution of carbonate of soda and chlorid of ammonium and if the latter be present in considerable quantity there will be no precipitate even on boiling. When the chlorid of ammonium is present in smaller quantity there is a point where the addition of a drop of carbonate of soda produces a cloudiness which clears up on heating.

(3.) If in the last experiment chlorid of sodium be substituted for chlorid of ammonium a similar action may be observed although it is much less in degree. In one experiment in which an excess of carbonate of soda was used and a partial precipitate produced in the cold, the liquid was left in repose during twenty-four hours, but on being filtered and the clear filtrate boiled, an additional amount of carbonate of baryta separated.

(4.) When a mixed solution of carbonate of soda and nitrate of potash is quickly added, in large excess, to a small quantity of a solution of chlorid of barium or of hydrate of baryta, no immediate precipitate is produced except on boiling.

The most remarkable solvent action which I have noticed is seen in the inability of the alkaline carbonates to precipitate baryta, and especially lime, from their solutions, when added in great excess. That such solvent power exists may be proved by precipitating a small quantity of a salt of lime with carbonate of ammonia and then redissolving the precipitate in a very great excess of the precipitant. But a much more satisfactory proof may be obtained by adding quickly a large excess of the solution of the alkaline carbonate to a small portion of a dilute solution of a lime or baryta salt; so quickly that the precipitate

may not have a sufficient time to form. This is readily accomplished by swinging rapidly the vessel containing the solution of the lime or baryta salt and suddenly turning into it the solution of the alkaline carbonate. If the solutions have been used in proper proportion no trace of a precipitate will appear, owing to the complete mixture obtained by this method of experimenting, although a fractional amount of the lime salt used would have produced a persistent precipitate had it been slowly added. By using the same portion of the solution of the alkaline carbonate upon successive small portions of that of the lime salt no inconsiderable amount of the latter may be finally obtained in solution. As a rule this solution is precipitated at once on boiling, but if it be diluted with a large quantity of water, ebullition no longer produces any precipitate. In one instance such a solution left in repose during twenty-four hours afforded no precipitate, although oxalate of ammonia when added to it produced a copious precipitate of oxalate of lime.

I have detailed but a few of the many cases where the presence of alkaline salts exerts an influence to retain lime or baryta in solution simultaneously with carbonic acid, but enough has been said to show that this power is quite general and that the salts of potash and of soda act in this respect like those of ammonia.

That the power of the alkaline salts to retain the carbonates of lime and baryta in solution may be referred to affinities analogous, in kind, to those which cause them to form double salts with the compounds of zinc, lead and magnesia, is, I think, evident. It is true that the affinities here are much weaker and insufficient to produce stable crystalline compounds. As in many other cases of double elective affinity, some shock, such as agitation or the application of heat, is required to call a desired play of affinities into action,—so here the affinity of the alkaline salt for the carbonates of the alkaline earths is sufficient to retain the latter in solution until heat is applied.

The well known fact that a current of carbonic acid gas produces no immediate precipitate in a solution of a salt of lime or of baryta neutralized by ammonia seems to depend on a mixed action: in part, like that previously alluded to, which prevents the precipitation of carbonate of baryta when a current of carbonic acid gas is passed through dilute baryta water, and also on the tendency of the carbonates of lime and baryta to form compounds with the salts of ammonia.

I find that a weak solution of caustic soda, potash or even lime may be substituted for caustic ammonia in the above mixture with like result, no precipitate appearing until after the lapse of considerable time unless the solution be heated. The action of the fixed alkali being, to all appearance, en-

tirely analogous, in kind, to that of ammonia, although less in degree.*

To demonstrate this it is only necessary to employ sufficiently dilute solutions of the caustic alkalies and to pass through the mixture a stream of carbonic acid gas diluted with air,—air expired from lungs, for example,—when no immediate precipitate will be produced unless the solution be heated. Even if the solution of caustic alkali be used in so concentrated a form (not sufficiently so however to precipitate a hydrate of the alkaline earth) that a precipitate of carbonate of lime is produced, in the cold, by a current of carbonic acid, it can readily be proved that a portion of the carbonic acid has not been precipitated, by filtering and boiling the clear filtrate, when a copious precipitate of carbonate of lime will be produced at once. This behavior is more marked with lime salts than with those of baryta, and soda evidently exerts a greater influence than potash.

If a solution of chlorid of sodium, of chlorid of potassium, or of chlorid of ammonium be added to the mixed solution before passing carbonic acid gas the precipitation of the carbonate of lime or baryta is attended with still greater difficulty.

Attention has been called by several observers (among others Rant, *J. pr. Chem.*, ii, 440; Vogel, *ibid.*, vii, 453) to errors which may arise in the course of analysis if the solvent action of chlorid of ammonium upon the carbonates of lime and of baryta be not kept in view. The remark applies with similar force to most of the soluble alkaline salts. This is of special importance in the determination of carbonic acid by means of a solution of chlorid of calcium neutralized with caustic ammonia. As a remedy it has been suggested (Kolbe, *loc. cit.;* Mohr, *Titrirbuch*, i, 110 and 113) that the solution should be boiled, in order to throw down all of the carbonate of lime. This would, it is true, in most cases cause the entire precipitation of the carbonate. But it is possible, especially in the analysis of some mineral waters, that alkaline salts may be present in sufficient quantity to prevent the precipitation of a portion of the carbonate of lime even on ebullition.

* Kolbe (*Handwoerterbuch der Chem.*, I Supplem., S, 157,) explains the behavior of the solution of chlorid of calcium or barium neutralized with ammonia, by supposing that an organic compound carbamate of ammonia, $NH_4O, C\left\{\begin{matrix}O\\AdCO_2\end{matrix}\right.$ is formed, capable of existing for some time in cold aqueous solution but not of supporting heat, on application of which it is transformed into ordinary carbonate of ammonia which reacts at once on the lime or baryta salt present. The insufficiency of this view to explain all the facts in the case is evident: (1.) In presence of an excess of chlorid of ammonium, carbonic acid produces no precipitate in the mixed solution of chlorid of calcium and ammonia even when it is boiled. (2.) When a large excess of any alkaline carbonate is added to a small amount of a solution of a salt of lime or of baryta, no immediate precipitate is produced unless the solution be heated. (3.) A current of carbonic acid gas produces no immediate precipitate in a solution of chlorid of calcium or chlorid of barium which has been neutralized with a dilute solution of caustic soda or potash instead of ammonia.

Bearing on this subject is the fact noticed by Denham Smith (*Lond. and Edin. Phil. Mag. and Journ. of Science*, ix, 542), that the carbonates of lime and of baryta are rapidly decomposed when boiled with solutions of the ammoniacal salts, carbonate of ammonia being evolved. This appears to have been overlooked by the above cited observers, but the fact can be easily proved by adapting an abduction tube to a flask containing a solution of chlorid of ammonium and some carbonate of lime, boiling the mixture and passing the vapor generated into lime water, when a copious precipitate of carbonate of lime is at once produced, and this in spite of the retarding influence of the ammonia present. As carbonate of baryta appears to be affected in much less degree than carbonate of lime by the solvent action of the alkaline salts, and to be capable of separating entirely in the cold from such solution after sufficient time, the use of chlorid of barium in determining carbonic acid would seem to yield more accurate results than that of chlorid of calcium.

ART. VI.—*On the Heights of the Tides of the Atlantic Coast of the United States, from observations in the Coast Survey;* by A. D. BACHE, Superintendent.—With a Plate.

[Communicated by authority of the Treasury Department to the American Association for the Advancement of Science.]

IT is well known that when a bay or indentation of the coast presents its opening favorably to the tide wave, and decreases in width from the entrance towards its head, that the tides rise higher and higher from the mouth upwards. The Rev. Mr. Whewell has stated that in a general way the same fact is deduced from the observations on the coast of Great Britain and Ireland, discussed by him.

The Coast Survey observations of the tides of the Atlantic coast, the results of which, from time to time, I have brought before the Association, furnish the means of a complete discussion of heights as well as of times, and very simple generalizations result from their examination. Through the kindness of Captain Shortland, R. N., and of Admiral Bayfield, R. N., I have been enabled to extend these results to the coasts of New Brunswick, Nova Scotia, and to part of Newfoundland.

I beg leave to make my best acknowledgements to these distinguished hydrographers for the prompt and liberal communication of the results of their observations.

The Coast Survey observations have been worked up in the tidal division under the direction of Assistant L. F. Pourtales, and I am indebted to him for giving the results the shape desired and for the diagrams representing them.

The following table of stations on or near the exterior coast-line of the United States, is taken from the more extended tables of the Coast Survey, omitting stations which are up rivers or bays, except in special cases the object of inserting which will be obvious.

Table A contains a number for reference, the locality of the tidal station, the state to which it belongs, the latitude, the longitude, and the mean height of the tide in feet and tenths.

TABLE A.

Heights of Tides on the Atlantic Coast of the United States.

No.	Locality.	State.	Lat.		Lon.		Heights in feet.	No.	Locality.	State.	Lat.		Lon.		Heights in feet.
			°	′	°	′					°	′	°	′	
1	Portland,	Me.	43	39	70	14	8·8	22	Fire Island,	N.Y.	40	38	73	13	2·1
2	Portsmouth,	N.H.	43	04	70	42	8·6	23	Sand's Point,	"	40	52	73	43	7·7
3	Newburyport,	Mss.	42	48	70	52	7·8	24	Sandy Hook,	"	40	28	74	00	4·8
4	Gloucester,	"	42	37	70	40	8·9	25	Cold Spring Inlet,	N. J.	38	57	74	45	4·4
5	Salem,	"	42	31	70	54	9·2	26	Cape May,	"	38	56	74	57	4·8
6	Boston,	"	42	22	71	03	10·0	27	Old Pt. Comfort,	Va.	37	00	76	18	2·5
7	Plymouth,	"	41	57	70	40	10·1	28	Hatteras Inlet,	N. C.	35	12	75	43	2·0
8	Provincetown,*	"	42	03	70	11	9·2	29	Beaufort,	"	34	42	76	40	2·8
9	George's Sh'ls,†		41	40	67	45	7·0	30	Cape Fear,	"	33	52	78	00	4·4
10	Monomoy,	Mss.	41	33	69	59	3·8	31	Winyah Bay,	S. C.	33	14	79	8	3·8
11	Siasconsett,	"	41	15	70	00	2·2	32	Charleston,	"	32	46	79	54	5·0
12	Weeweeder,	"	41	15	70	05	1·2	33	N'th Edisto River,	"	32	33	80	13	5·8
13	Smith's Point,	"	41	17	70	16	2·1	34	Port Royal,	"	32	17	80	40	7·0
14	Wasque,	"	41	21	70	30	1·7	35	Savannah Entr.	Ga.	32	02	80	53	7·0
15	Menemsha,	"	41	20	70	45	2·7	36	Sapelo,	"	31	21	81	24	6·6
16	Point Judith,	R. I.	41	22	71	29	3·1	37	St. Simons,	"	31	8	81	36	6·8
17	Newport,	"	41	29	71	20	3·9	38	St. Mary's River,	"	30	42	81	36	5·9
18	Block Island,	"	41	10	71	34	2·8	39	St. John's River,	Fla.	30	20	81	33	4·6
19	Montauk Point,	N.Y.	41	04	71	51	1·9	40	St. Augustine,	"	29	52	81	25	4·2
20	Stonington,	Ct.	41	20	71	54	2·3	41	Indian River Inlet,	"	27	28	80	19	2·5
21	New Haven,	"	41	18	72	54	5·8	42	Cape Florida,	"	25	40	80	09	1·5

The following table of tides of localities on the coast of Cape Breton, Nova Scotia and New Brunswick, is from the observations of Admiral Bayfield and Captain Shortland. The authorities are given in the column of remarks, which also contain the remarks of Admiral Bayfield on the tidal results communicated by him. I have taken from his table the heights which were derived from the greatest number of observations. The column of means is the average of the heights of spring and neap tides in feet and tenths. The localities are arranged from the north southward on the outer coast; and in the Bay of Fundy from the entrance up the bay.

From the table of Captain Shortland I have selected only a few localities as specimens, having no wish to anticipate, through his generosity, the use which he will doubtless make of his own results.

* Major Graham, U. S. A. † Capt. Wilkes, U. S. N.

APPROXIMATE COTIDAL LINES

SAILING LINES AND LINES OF EQUAL HEIGHT OF TIDES

of the

Atlantic Coast of the United States

From

investigations in the Coast Survey

A.D.Bache Supd^t

Scale 1/1.000.000

1857

Notes.

XII ——— Denote the Approximate Cotidal Lines

XI½ ———

XI ——— and the Roman numerals the Cotidal Hours in Greenwich time

Curves of 30 Fathoms water

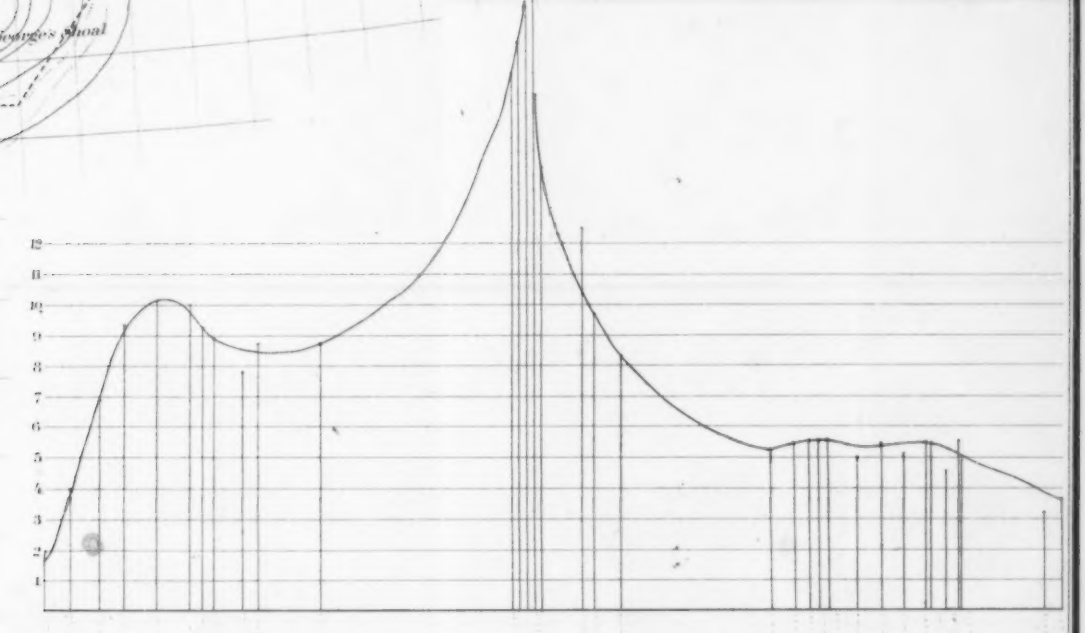
100

24 ft ——— Lines of equal height of tides

34 ft ———

---------- Sailing Lines.

Note. Diagram



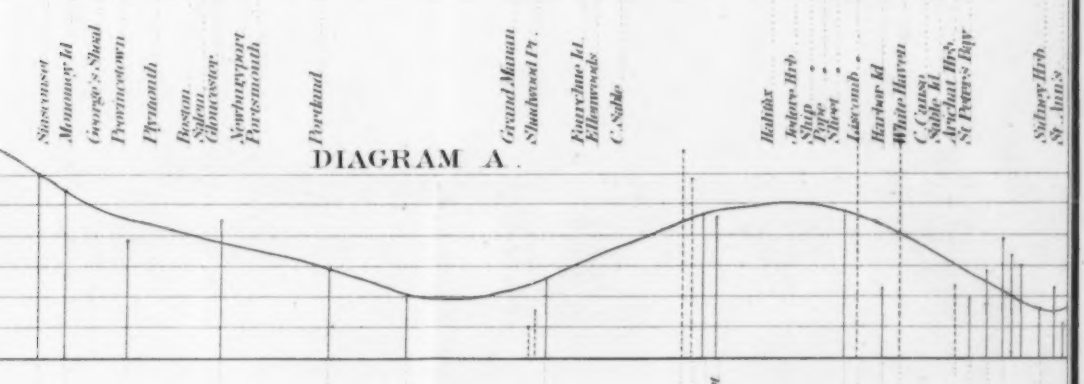
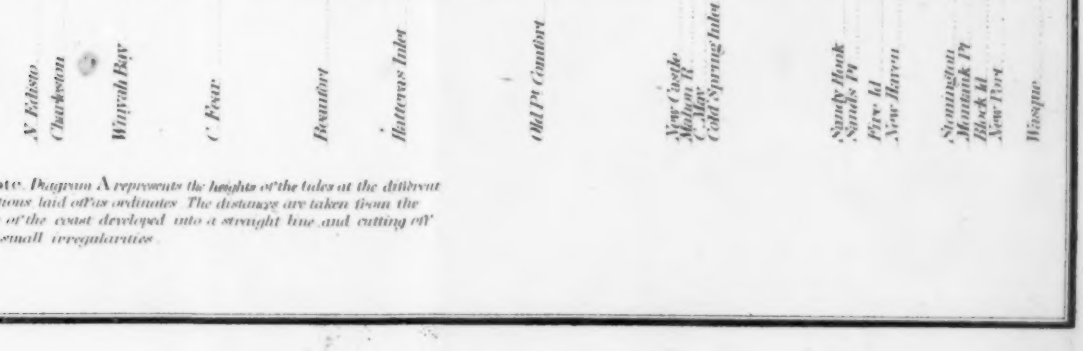


DIAGRAM A.



ote. Diagram A represents the heights of the tides at the different
ious laid off as ordinates. The distances are taken from the
of the coast developed into a straight line and cutting off
small irregularities.

TABLE B.

Heights of Tides on the Coast of Cape Breton, Nova Scotia and New Brunswick.

No.	Localities.	Remarks on Localities.	Lat. °	Lat. ′	Lon. °	Lon. ′	Rise of Tide. Ord. Spring ft.	Ord. Spring in.	Ord. Neap. ft.	Ord. Neap. in.	Mean. ft.	Remarks and authorities.
	Is'd of Cape Breton.											Admiral Bayfield.
1	St. Ann's Harbor, .	Entrance,	46	17	60	33	5	0	3	3	4·1	A complete semi-lunation observed.
2	Sydney Harbor, . .	S. E. Bar,	46	12	60	13	3	9	2	4	3·1	At full moon and a day or two before and after.
3	Menadou Harbor, .	Near Scataria Island,	46	00	59	50	5	6	3	4	4·4	Good. A complete semi-lunation observed.
4	St. Peter's Island, .		45	36	60	49	6	0	4	0	5·0	At new moon and a day or two before and after.
5	St. Peter's Bay, . . .	Haul-over at head of Bay, . . .	45	39	60	52	5	9	4	1	4·9	Good observations, four times observed, twice at the full, and twice at the new moon, with several days before and after.
6	Grandigue, ...	In Lennox passage, . .	45	36	61	01	6	4	4	6	5·4	Good, a complete semi-lunation observed.
7	Arichat Harbor, . . .	Jerseyman Isd, N. poi't,	45	30	61	03	5	0	4	0	4·5	ditto. ditto. ditto. Extraordinary tides, rise 6 feet.
	Nova Scotia.											
8	Canso Harbor, ...	E. end of Cutler's Is'd.	45	21	60	59	6	6	4	6	5·5	A complete semi-lunation observed, but tides very irregular.
9	White Haven, ...	Marshal Cove, ...	45	15	61	11	6	1	4	1	5·1	A complete semi-lunation observed, good observations.
10	Harbor Island, . . .	N. E. Point, ...	45	08	61	36	6	6	4	6	5·5	A complete semi-lunation observed, extraordinary tides, rise 7 feet.
11	Liscomb Harbor, . .	Pye's Wharf, ...	45	00	62	01	6	0	4	0	5·0	Three times observed at full and new moon, and several days before and after.
12	Sheet Harbor, ...	Watering Cove, ...	44	54	62	30	6	8	4	6	5·6	Good—two complete semi-lunations observed.
13	Pope Harbor, ...	Harbor Isd., N.E. Point,	44	48	62	39	6	6	4	2	5·3	Three times observed at full and new moon.
14	Ship Harbor, ...	Salmon Point, ...	44	47	62	49	6	5	4	10	5·6	Good, a complete semi-lunation, extraordinary spring-tides, rise 7 feet, and extraordinary neaps, only 4 feet.
15	Jedore Harbor, . . .	Marsh Point, ...	44	43	63	00	6	6	4	8	5·6	Two good and complete semi-lunations observed.
16	Halifax Harbor, . . .	Naval Yard, ...	44	40	63	35	6	0	4	6	5·2	Mean of a complete year's observations with a tide gauge. Captain Shortland, R. N.
	Bay of Fundy.											
17	Cape Sable, ...	Cape Sable Island, Clarke's Harbor, . . .	43	25	65	39	11	6	4	11	8·2	
18	Ellenwood's Island,	Bird Rock, ...	43	39	66	04	12	7	7	0	9·7	
19	Yarmouth Harbor,	Fourchue Isd. lighth'se,	43	47	66	10	10	9	16	0	13·3	
20	Bryer's Island, ...	Peter's Isd. lighthouse,	44	15	66	21	20	6	9	3	14·8	
21	Campbell's Island, .	Owen's House, ...	44	54	66	58	25	0	11	0	18·0	
22	St. John's, N. B. . . .	Battery Point Rock, . .	45	16	66	04	26	8	12	0	19·3	
23	Shadwood Point, . .	Cumberland Basin, . . .	45	54	64	22	50	0	22	0	36·0	

These numbers may be extended beyond the turn of Cape Race, where the coast trends to the west of north, by further results of Admiral Bayfield, though the remarks which he makes show them to be only approximate. Thus two stations on the coast of Labrador, St. Lewis Bay in latitude 52° 19′ and longitude 55° 37′, and Henley Island in latitude 52° 00′ and longitude 55° 53′, give each for the mean of the height of spring and neap tides 2·3 feet. St. John's, Newfoundland, gives 5·0 feet. Trepassey harbor, south of it, 5·8 feet.

Beginning with the southern end of table A and following the results northward and eastward, we find from Cape Florida to Savannah and Port Royal a gradual increase of the tides, and then a gradual decrease to Cape Hatteras, with a single contradiction easily explained. Next, following the stations on the coast, and omitting those in the bays and sounds, we have a less regular increase to Sandy Hook, and a decrease to Weeweeder on Nantucket Island. Next is a less regular regimen requiring a more detailed examination.

By developing the curved line of the coast into a straight line and marking upon it the tide stations, which will be thus at nearly their proper distances from each other, and by erecting ordinates at each of the station points and setting off on a suitable vertical scale the heights of the tides at those points, and connecting the extremities of the several ordinates, we have the broken line shown in diagram A. In drawing this line the stations of the coast only are joined, and the irregularities are cut off by the curve.

This curve shows distinctly the *physical* division of the coast between Cape Florida and Cape Sable into three great bays. The great southern from Cape Florida to Cape Hatteras, the great middle from Cape Hatteras to Siasconsett, the great eastern from Siasconsett to Cape Sable. Perhaps this latter may be considered as only a portion of a great bay from Siasconsett to Cape Race; but this generalization is at present hardly safe, and I confine myself therefore to the more limited view. The tide wave setting into the southern bay, rises as the bay contracts, and the heights of the tides along the shores increase as the places are more distant from the chord spanning the entrance.

If we suppose the lines of equal height to be straight lines and draw them upon the diagram, transferring them to a map of the coast, we shall find that they are more crowded on the more curved side and more open on the less curved. The curve indicates Cape Hatteras, and not the inlet, which was the tidal station, as the point of least height. The physical cause of this phenomenon is well understood if it has not yet been reduced to measure.

The next curve shows us plainly the middle bay, having Hatteras for its southwestern cape, and Smith's Point or Weeweeder

for its northeastern entrance. The form of the shore is less favorable to regularity, but the result is nevertheless well marked. The interference of tidal waves, which takes place off Nantucket, tends also in a degree to confuse the results. The chart shows how simple the system of cotidal lines is in the three bays running nearly parallel to the shores.

The eastern bay lies between the eastern part of Nantucket (Siasconsett) and Cape Sable, Massachusetts bay being subsidiary to this. The tide wave entering the eastern bay follows the deep water, and thus the cotidal lines take generally the directions of the shores until the tide wave enters the Bay of Fundy. The most probable form of the cotidal lines from XI to XV hours inclusive is shown upon the chart, which is merely an extension of the chart of cotidal lines of the United States coast formerly presented to the Association. The heights increase rapidly from Nantucket to Cape Cod, being 2 feet at Siasconsett and 9·2 feet at Provincetown. At Cape Ann they are nearly of this same height and increase in passing up and into the bay to 10·0 feet at Boston and 10·1 feet at Plymouth.

The height at Newburyport is probably local, depending upon the position of the tide-gauge. There is but little change from Portsmouth to Portland, and from Cape Sable to Ellenwood's Island.

Shall we look to the greater bay between the Nantucket and Newfoundland shoals for the cause of the eight feet rise at Cape Sable, and of the heights from Admiral Bayfield's table. We find the heights along the coast of Nova Scotia to vary from seven to six feet, not with regularity however. At Cape Breton Island they vary from 6·4 to 4·6 feet, decreasing thus in going northward and eastward. Are these heights due to the crowding of the waters into this greater bay? If so, why are not the heights of Cape Breton greater than those of Nova Scotia? We require results on the south shores of Newfoundland and on the Great Bank to give us clear ideas on these points, and I hesitate to extend the generalization to this tempting field.

The shoals from Nantucket and broken ground near George's Bank and the comparatively shoal water in their vicinity on the one side, and the Great Bank of Newfoundland on the other, look as if full of meaning of this sort. Further results may however show that this is not the interpretation of the phenomena. The tides of Labrador are but 2·3 feet, bringing us back to the standard of Hatteras and of Montauk Point, and what probably would be that of Nantucket but for interferences.

Soon after passing Mt. Desert on the west side and Ellenwood's Island on the east side, the tide wave has turned into the Bay of Fundy, and the rise increases with extraordinary rapidity. The complicated character of the cotidal lines in this immediate

vicinity is indicated by the chart, the lines from XII to XV hours being crowded into the very small space of a few miles on the south side of Nantucket.

To return to the more limited scale within which our inductions are safe, Delaware bay, New York bay, Long Island sound, Narragansett and Buzzard's bays, Nantucket and the Vineyard sounds, present on a smaller scale the same phenomena of increase in the height of the tide in ascending. On the contrary, in Chesapeake bay, which widens and changes direction at a right angle immediately from the entrance, the tides diminish in height as a general rule in going up the bay.

The results of the heights of tides along the coast are very satisfactorily shown upon a model which is now before the Association, for superintending the execution of which I am indebted to Mr. Pourtales. The basis is a map of the Atlantic coast from Cape Florida to Cape Race, upon which the cotidal lines of the United States are traced. The tidal stations are marked upon this, and rods cut to length and proportionate to the rise and fall of the tides at the several stations, are inserted in holes drilled at the station points. The steel rods refer to the heights at exterior stations and the brass rods to interior ones. Paper cut to the form of the general curve of heights, which has already been explained, and placed behind these rods, serves to show the generalizations with great distinctness.

I propose to call the bay between Cape Florida and Cape Hatteras the Southern bay, that between Cape Hatteras and Nantucket the Middle bay, and that between Nantucket and Cape Sable the Eastern bay of the coast of the United States.

The general figure of the coast line has of course heretofore attracted the attention of geographers. The connection with the heights of the tides could only satisfactorily be made out by such a series of tidal observations as those embraced in the Coast Survey.

ART. VII.—*On the Winds of the Western Coast of the United States, from observations in connection with the U. S. Coast Survey;* by A. D. BACHE, Superintendent.—With a Plate.

[Communicated by authority of the Treasury Department to the American Association for the Advancement of Science.]

THE observations, of which I propose at present to communicate the results, were made in the year 1855, in connection with the tidal observations on the Pacific coast, at three permanent stations, Astoria, San Francisco, and San Diego. The approximate latitude and longitude of each of the stations is as follows:

Astoria, Oregon,	lat.	46°	11′	N., long.	123°	49′	W.
San Francisco, California,	"	37	48	"	122	28	
San Diego, "	"	32	40	"	117	12	

U.S. COAST SURVEY

A.D. BACHE Superintendent

MEASUREMENT OF EPPIN

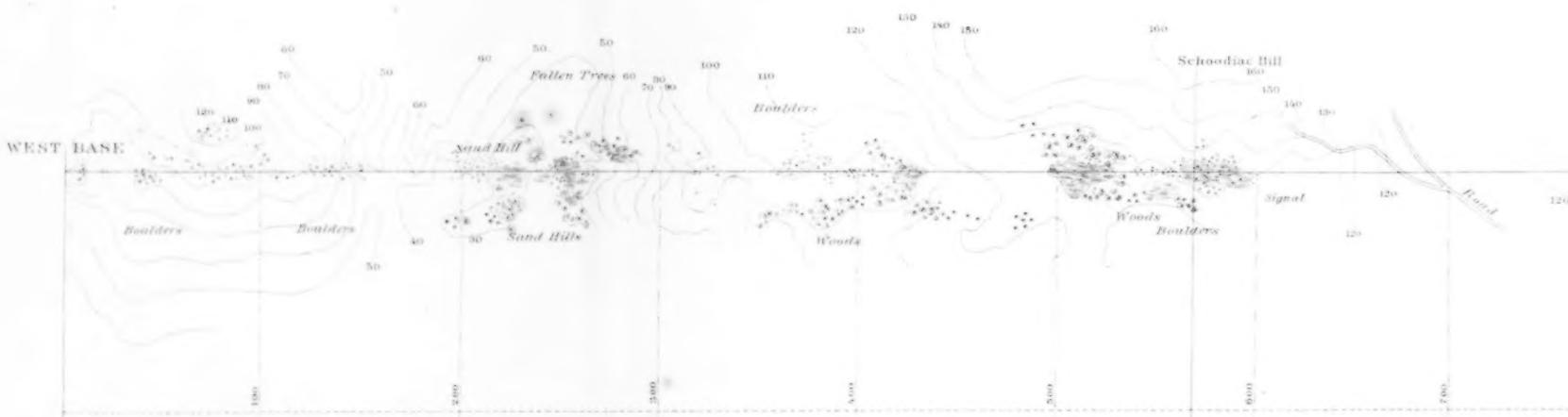
MAINE.

Scale $\frac{1}{20\,000}$

1857.

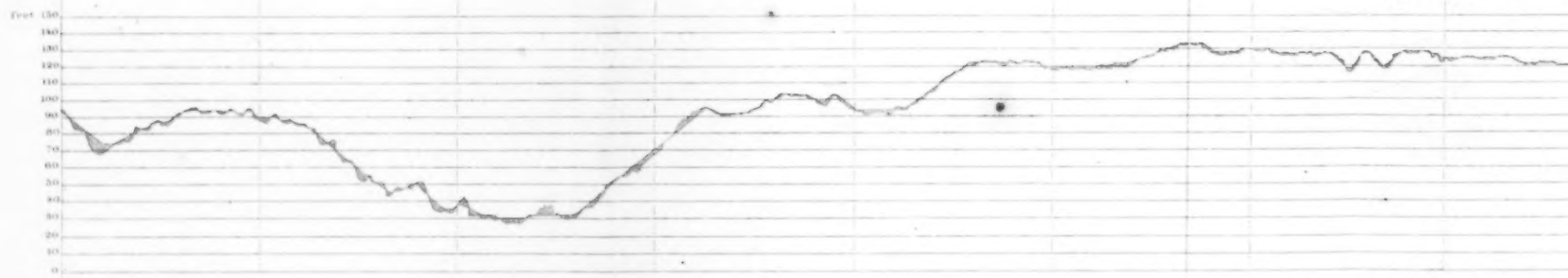
PLAN OF EAST BASE GRANITE MONUMENT

shewing its relation to the base and side monuments
and the feet of surrounding Tripod and Scaffold.



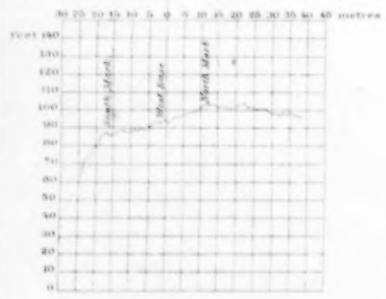
Distances in tubes fro

PROFILE OF BA

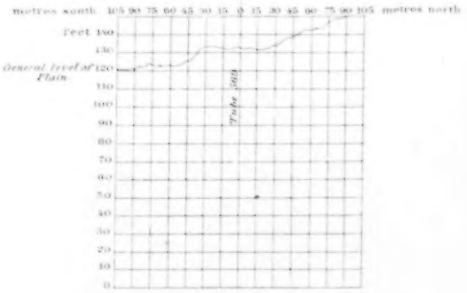


The Datum line is 100 feet below be

CROSS SECTION AT WEST BASE



CROSS SECTION AT SUMMIT



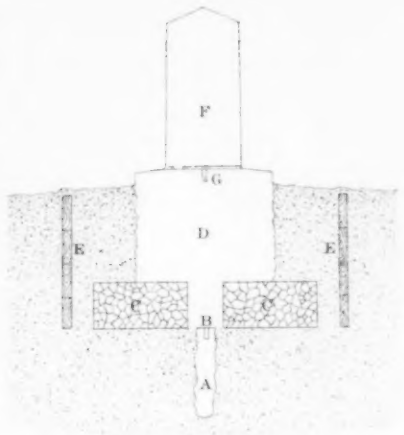
Lith. of J. Bien 60 Fulton St. N.Y.

Nº I. PLAN, SECTIONS

RVEY

endent

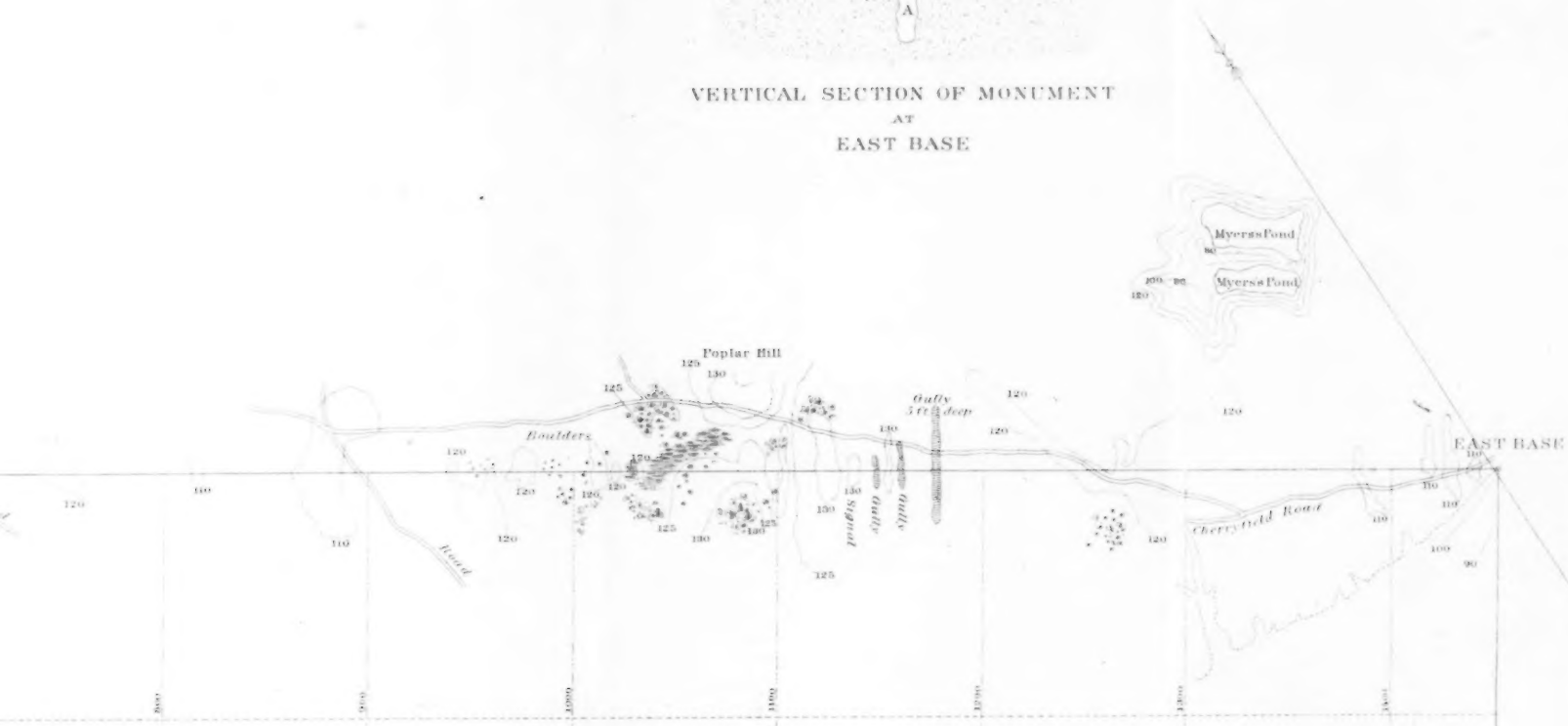
EPPING BASE



VERTICAL SECTION OF MONUMENT
AT
EAST BASE

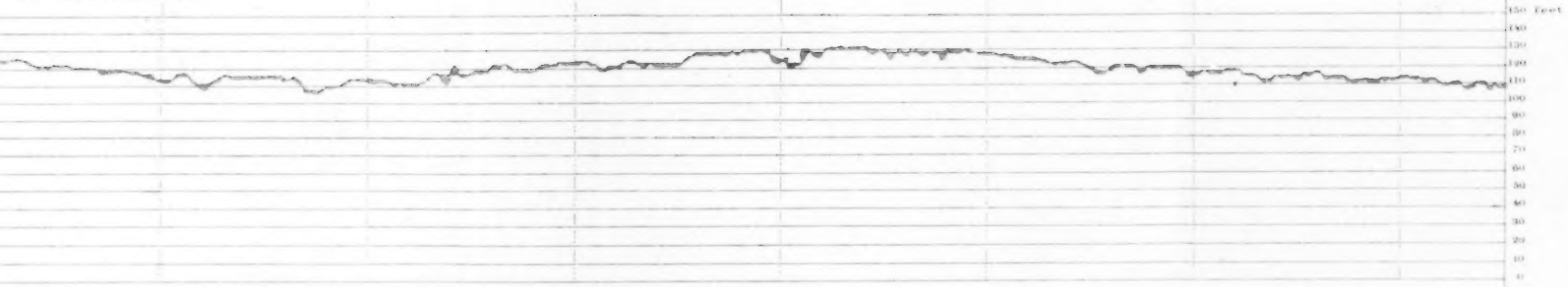
Explanations.

A. *Granite Post, top 3.5 ft. below surface.*
B. *Copper Bolt with cross on head, marking Terminus.*
C.C. *Granite Platform laid in Cement; Five feet square by one foot high, with square hole in centre.*
D. *Granite Block 3 feet square by 2.5 feet high, weighing 3600 lbs.*
E.E. *Sides of Plank Curb, Six feet square.*
F. *Marble Monument with inscriptions.*
G. *Copper Bolt with cross, marking Terminus.*



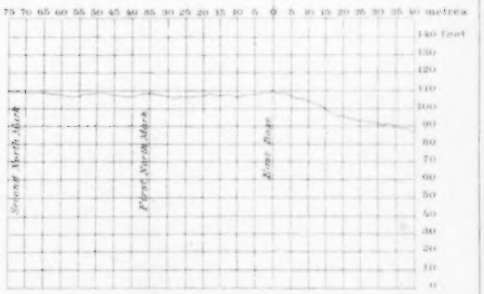
ubes from West Base.

OF BASELINE.



below bench mark at East Base.

CROSS SECTION AT EAST BASE



Note for Profile

Excavation is marked by horizontal shading
Embankment " " vertical shading.

TIONS AND PROFILE.

The mode of observing was that described in my paper on the winds at Cat Island, read before the Association in 1850. The observers were posted and practised together by Lieut. W. P. Trowbridge of the U. S. Corps of Engineers, under whose supervision the observations were made.

The directions of the wind were noted in points, and the force in the conventional scale before referred to. These numbers were reduced to velocity in miles per hour by the tables given in my former paper, and the quantity of wind blowing from any quarter during a given period was thence readily found. The tables and diagrams are thus of the same kind as those which I have before presented to the Association. They were made under the direction of Assistant L. F. Pourtales of the U. S. Coast Survey, to whose care, assiduity and knowledge I am indebted for the opportunity of presenting them. The computations and the diagrams were made by Miss Mary Thomas.

The observations were taken three times each day, at 6 A. M. and P. M. and at noon, except on Monday of each week when hourly observations took the place of the regular daily ones. From these latter results the reference of the three daily observations to the mean of the day has been made. The quantities of wind for each hour and for each direction were computed and grouped by months and then plotted. The eye readily takes in the characteristics of the winds at different periods of the day and year and for the various directions. To apply these to the reduction of the daily observations, tables were formed of the average time during which each wind blowing would give from observations at the three hours already named the result for the day. For example, the W. wind at San Francisco gave for the quantity in twenty-four hours by the daily observations 505, the mean hourly quantity at 6 A. M. being 6, at 12 M. 27, and 6 P. M. 31. These quantities respectively being supposed continued for nine hours, five hours, and ten hours, which agrees with the diagram, would give 499, a number differing but little from the total found for the day.

Table for deducing from the three daily observations the mean of the day.

ASTORIA.				SAN FRANCISCO.				SAN DIEGO.			
Wind.	6 A. M.	12 M.	6 P. M.	Wind.	6 A. M.	12 M.	6 P. M.	Wind.	6 A. M.	12 M.	6 P. M.
N.E.	6h	6h	6h	N. & N.E.	9h	6h	9h	N. & N.E.	6h	6h	6h
E. (Oct. to Mar. incl.)	9	6	9	E.	3	3	3	E.	3	3	3
E. (Apr. to Sept. incl.)	3	3	3	S.E., S. & S.W.	9	6	9	S.E. & S.	9	6	9
S.E.	6	6	6	W.	9	5	10	S.W. & W.	18	6	6
S.	9	6	9								
S.W. & W.	9	6	9	N.W.	9	6	9	N.W.	18	7	6
N.W.	8	5	7								
N.	3	3	3								

In this way the above table was found, which was applied to the reductions of the daily observations.

From the tables of velocities in miles per hour deduced from the observations by the method just explained, the following table of quantities of wind from different directions for each

QUANTITIES OF WIND.

ASTORIA.

Month.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	
January,	6	795	987	150	324	2719	1539	....	6520
February,	138	282	1623	295	3079	225	850	....	6492
March,	....	....	1253	509	933	585	428	....	3708
April,	24	180	305	282	1218	969	1651	1801	6430
May,	....	102	105	186	536	1437	876	1393	4635
June,	....	60	33	....	9	258	2104	2760	5224
July,	....	150	18	....	....	633	700	4221	5722
August,	....	6	12	60	30	279	180	2543	3110
September,	....	54	....	102	872	....	246	431	1705
October,	....	754	294	....	1245	291	123	361	3068
November,	....	583	182	399	2252	2927	348	447	7138
December,	....	1535	456	693	2849	138	105	91	5867
	168	4501	5268	2676	13347	10461	9150	14048	

SAN FRANCISCO.

Month.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	
January,	2302	375	147	177	21	1471	264	1161	5918
February,	618	180	45	558	147	992	860	306	3706
March,	218	60	28	639	470	1934	2447	426	6222
April,	30	72	....	168	312	2992	4845	209	8628
May,	....	....	....	348	24	1578	4928	2530	9408
June,	....	....	....	....	18	8338	3428		11784
July,	....	....	....	....	9	8725	2020	362	11116
August,	....	....	....	....	....	2608	3908	2168	8684
September,	....	....	....	....	54	2588	4219	500	7361
October,	....	6	....	....	63	136	4396	96	4697
November,	630	252	....	231	232	508	2056	222	4131
December,	414	850	18	652	489	1290	1576	234	5523
	4212	1795	238	2773	1839	33160	34947	8214	

SAN DIEGO.

Month.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	
January,	91	147	45	431	813	384	643	987	3541
February,	204	48	207	1291	777	688	278	2461	5954
March,	284	114	114	1095	726	912	1124	3219	7588
April,	186	180	36	900	1019	798	810	2505	6434
May,	156	24	57	396	834	1296	930	2296	5989
June,	72	48	66	1100	827	882	892	2145	6032
July,	39	192	16	285	1320	607	120	4066	6645
August,	48	79	24	104	93	595	690	3823	5456
September,	48	186	39	54	265	338	426	2418	3774
October,	72	156	21	36	132	186	254	2411	3268
November		108	93	240	231	542	198	1226	2638
December,	114		24	162	345	686	528	754	2613
	1314	1282	742	6094	7382	7914	6893	28311	

month is found. The rhumbs are written at the top of the table, the months at the side, and at the meeting point of a vertical and horizontal line from the head and side titles are found the quantities. The last column at the side of the table gives the total quantities for the several months, and below is found the totals for each direction for the year.

From this table the diagrams representing the quantities for each month from each direction (figs. Nos. 1 to 12) are taken, and also those showing the total annual quantities of wind from each direction, and the total quantity of wind from all directions for each month (figs. Nos. 16 and 17).

It seems to me altogether probable from the study of the figures of these tables, that the scale adopted by the observer at San Francisco is greater than that at the two other points. The total quantities at Astoria, San Francisco and San Diego are as 59, 87, and 60, and it is hardly probable that there is so large an excess of quantity at San Francisco. I have also the same remark to make as on the observations at Cat Island, on the absence of observations upon the intermediate points between the cardinal ones, showing the tendency to designate the winds only by the cardinal points.

From these diagrams we see at once the simple general regimen of the winds on this coast.

1. The great prevalence of westerly winds, representing a flow of the air at the surface from the ocean in upon the land.

2. The general absence of easterly winds, showing the absence of a return current at the surface.

The proportion of westerly to easterly winds is as 8 to 1.

3. The increase of westerly winds in the summer and their decrease in the winter.

4. That when easterly winds blow at all, it is as a rule during the winter.

5. The N., N.E., and E. winds blow more frequently in the morning than in the afternoon hours.

6. The S.E., S., and S.W. winds are in general pretty equally distributed over the morning and evening hours.

7. The N.W. is the prevailing direction of the ordinary sea breeze at Astoria and San Diego, and the W. at San Francisco.

Sometimes the W. wind has that character at the first named stations and sometimes the S.W. wind at the last named.

A closer inspection of the same diagrams will lead to other interesting results.

Considering the qualities of wind at the three places for the whole year (fig. No. 13) San Diego and Astoria present remarkable similarities. There is more N.E., E., and S. winds at Astoria, and more N.W. wind at San Diego. The axis dividing the area symmetrically is in the same direction. On the contrary

at San Francisco the W. and S.W. winds give the character to the rose and the axis makes an angle of some sixty-seven degrees with that of the other spaces. All show the same deficiency of easterly winds, and San Francisco is deficient also in southerly ones.

The monthly curves grouped in two periods, from November to March, both included, and from April to October (figs. Nos. 14 and 15) show that the annual curve has the summer type impressed upon it. The summer is in fact the windy part of the year. The N.W. wind prevails in August at Astoria and San Diego, and the W. and S.W. at San Francisco.

The scale of diagram No. 14 is less than that of 15 in the proportion of 10 to 14. There is scarcely any wind from points between North, around by East and South. The form of the rose is exceedingly simple and the generalization very obvious.

The winter system is less simple. The axes of the spaces for Astoria and San Diego make angles of more than 110° with each other. The N.E., E., S., and S.W. winds are considerable at Astoria, and the N.W. wind is deficient. At San Francisco the W. winds give the prominent feature to the rose curve.

As the winter is not the windy season, so the months of March and September are not the windy months. On the contrary, July is one of the windiest months of the year.

San Francisco.

At San Francisco the great current of air flowing from the sea to the land comes generally from the W. or S.W., rarely from the N.W.

In the period from November to March inclusive, (diagram No. 14,) the W. is the prevailing wind, exceeding in quantity both the others, the S.W. wind exceeding in quantity the N.W. In the period from April to October (diagram No. 15) the W. and S.W. winds are nearly equal, and each exceeds the N.W.

The W. wind has in general the features attributed to the sea breeze, beginning after the rising of the sun, increasing until after the hottest part of the day, and dying out or much diminishing at nightfall.

The W. and S.W. winds give the prominent features to the wind rose at San Francisco.

The S.W. is the prevailing wind in June and July, S.W. and W. winds blowing nearly the whole of those months, not succeeded by an easterly land breeze—but rising and falling. The rose curves for May and August resemble each other, the N.W. and S.W. winds being nearly equal in quantity, and each less than the W. wind. So the curves for April and September, when the N.W. wind has nearly died out. The W. wind diminishes in quantity through March and February, and through

October, November, and December to January. The N.W. wind increases again from April towards December, and is very small in October and November. The S.W. wind disappears in October, changing the form of the rose curve, but reappearing in November and December and increasing towards January. The W. wind has a maximum in April and May and another in September and October, the minima being July and January.

The N. wind in December, January and February, reaching a maximum in January, is the only other point to be noticed for San Francisco, partaking with the other places in the general absence of easterly winds. The tables show a little in the winter. There is also but little S. wind there.

Astoria and San Diego.

In general the winds at these two places resemble each other more than those at San Francisco do either. The rose curves for April, May, June, July and August (Nos. 4 to 8) have the same general character. The mean curve for the year (No. 13) and for the summer period (No. 15) have also the same general character.

The N.W. wind is the summer wind and has the characteristics of the sea breeze, but there is no return land breeze. The N.W. wind reaches a maximum in July and a mininum in December. It is the great prevailing wind of the year (diagram No. 13) at San Diego. As it decreases it is generally replaced by W. and S.W. winds of less quantity. In December the quantities of the three winds are nearly equal.

The resemblance of these winds at San Diego and Astoria is remarkable, the remarks just made applying generally to both places. There is, however, much less N.W. wind at Astoria than at San Diego. Except in June, July and August there is some S. wind each month at Astoria, and especially from September through October, November, December and February, it presents a marked feature of the rose. At San Diego this is less marked, the two agreeing most nearly in quantity in March, April and May.

The S.E. wind is a distinct feature in both places in February and March, and at San Diego in April and June.

The E. wind is prominent at Astoria in January, February and March, and the N.E. from October to January inclusive.

Astoria has the most easterly wind, the N.E. beginning in October and blowing until February, and being replaced by the E. wind in March.

ART. VIII.—*Notes on the Measurement of a Base for the primary triangulation of the Eastern Section of the Coast of the United States, on Epping Plains, Maine;* by A. D. BACHE, Superintendent U. S. Coast Survey.—With a Plate.

[Communicated by authority of the Treasury Department to the American Association for the Advancement of Science.]

THE reconnoissance for a base of verification at the eastern extremity of the primary triangulation in Section I of the coast was commenced by Charles O. Boutelle, Esq., and Major Henry Prince, U. S. A., Assistants in the Coast Survey, in 1853 and continued through 1854 and 1855. The absence of long and straight sea beaches on this coast rendered it absolutely necessary to look for an interior site.

The reconnoissance resulted in the selection of Epping Plains, Washington Co., Maine, as the most suitable site for the purpose considering the character of the ground itself and the facility of connecting the ends of the base with the primary triangulation.

In this selection and the examination of the plains, these officers were much assisted by the local knowledge and the kind offices of J. A. Milliken, Esq., now of Cherryfield, to whom I beg leave here to return the thanks of the Coast Survey.

Major Prince being relieved from the survey, the final minute examination of the site and the determination of the best line which could be obtained on the plain, devolved upon Assistant Boutelle, who was assisted at different times by Sub-Assistant J. A. Sullivan, Lieut. J. C. Clark, U. S. A., and Mr. F. P. Webber.

Epping Plains, or "Barrens," as they are called, lie between the Narraguagus and Pleasant rivers. They present a moderately rolling surface of sand, generally destitute of trees, except in the lower and swampy parts, and are traversed by sand ridges of different elevations, resembling very much the surface which the sounding line develops, in such regions as the Nantucket shoals, at present below the surface of the water.

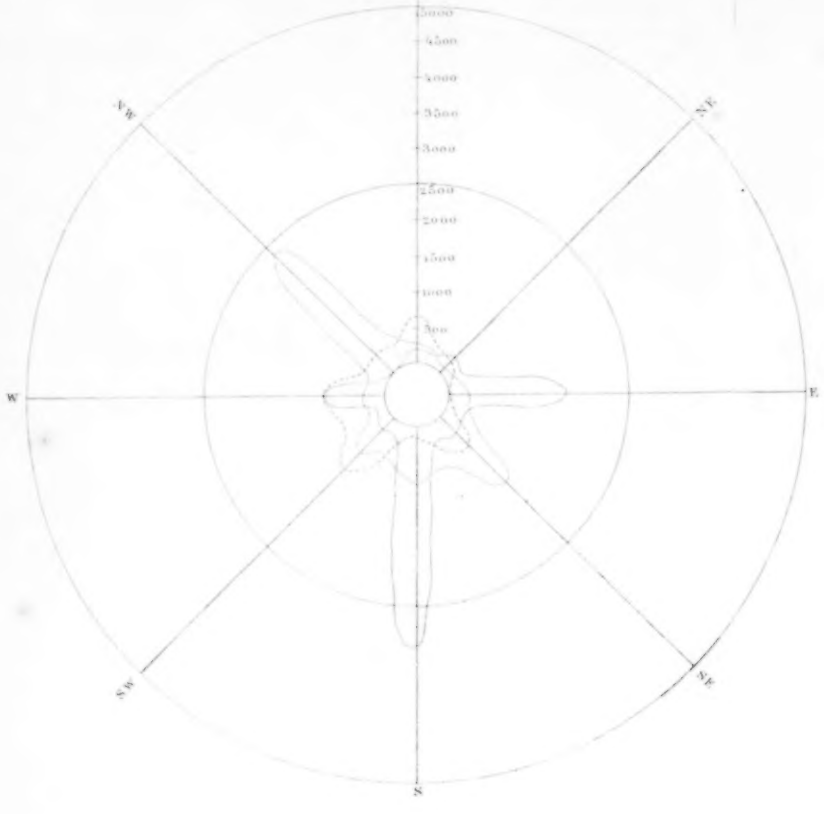
The plain is quite elevated and falls suddenly from an irregularly curved margin, by a steep slope to a lower plain or wide valley.

Portions of the plain are strewed with boulders of various sizes, some of them containing not less than 4000 cubic feet, and of various granitic materials. Schoodiac Hill was found to limit the position of the base, so that the problem became to draw the longest line through a point at the base of that hill, the ends of which would be easily visible from the secondary and primary stations.

Before the final selection of the line a topographical survey was made under the direction of Assistant C. O. Boutelle, by

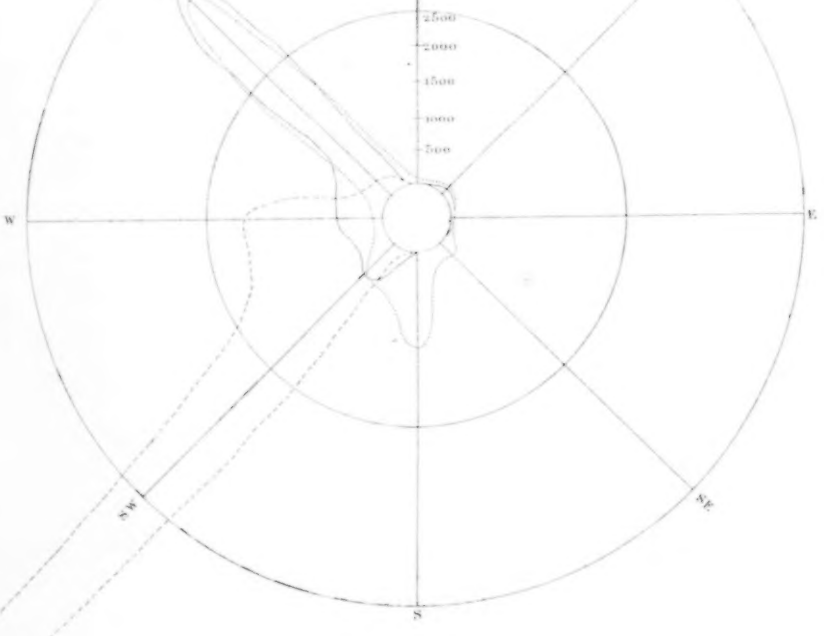
No. 1. JANUARY

No. 2. FEBRUARY



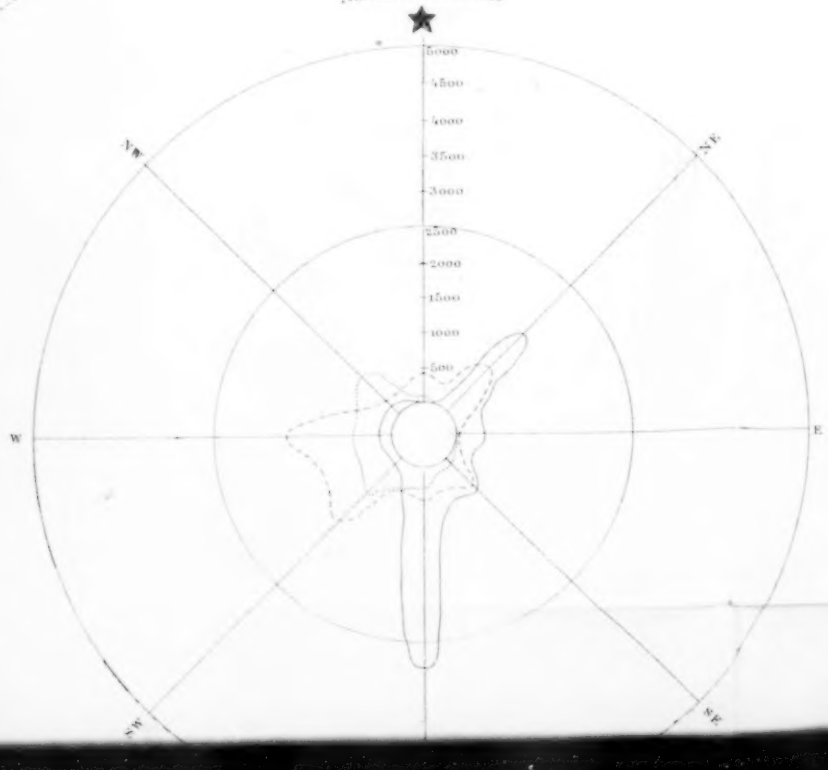
No. 6. JUNE

No. 7. JULY

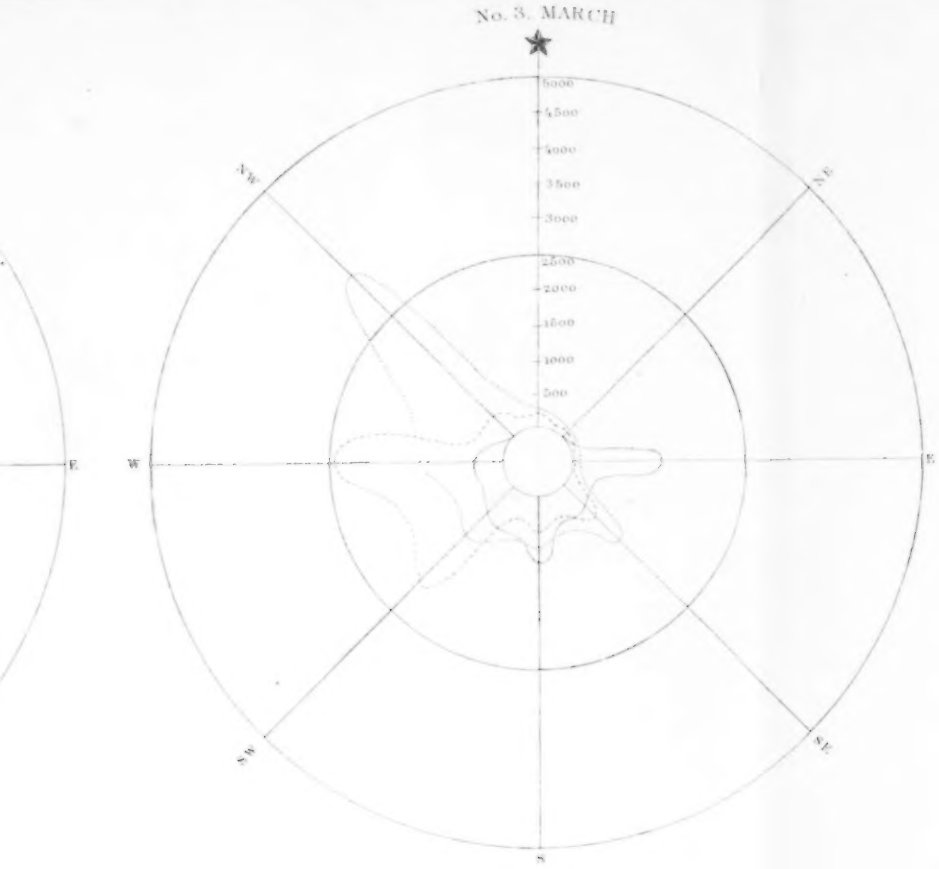


No. 11. NOVEMBER

No. 12. DECEMBER



No. 3. MARCH



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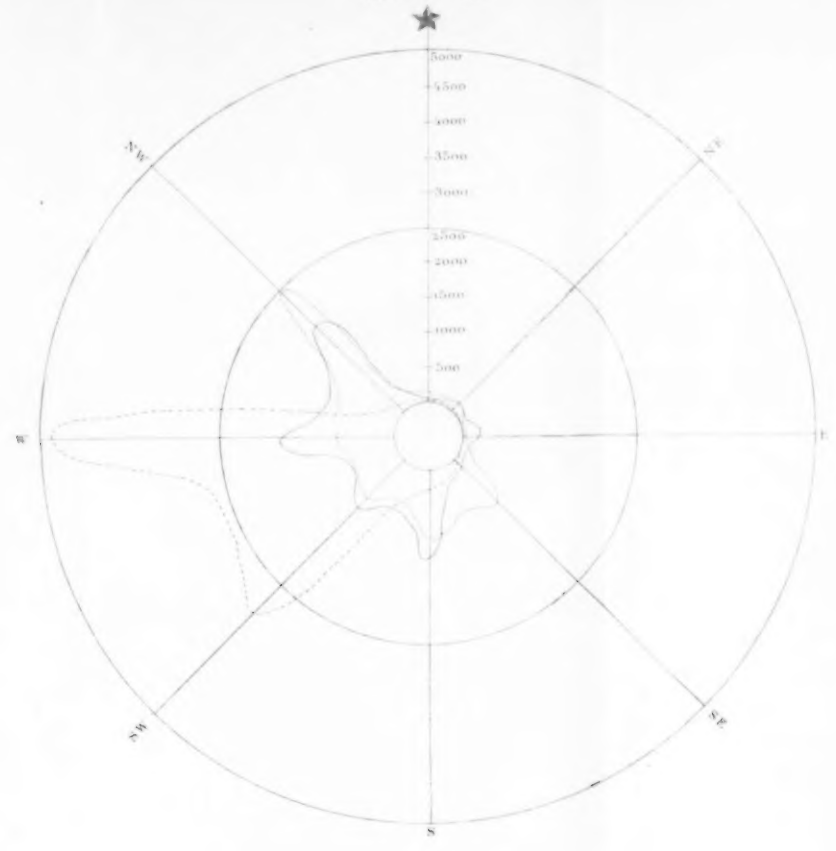

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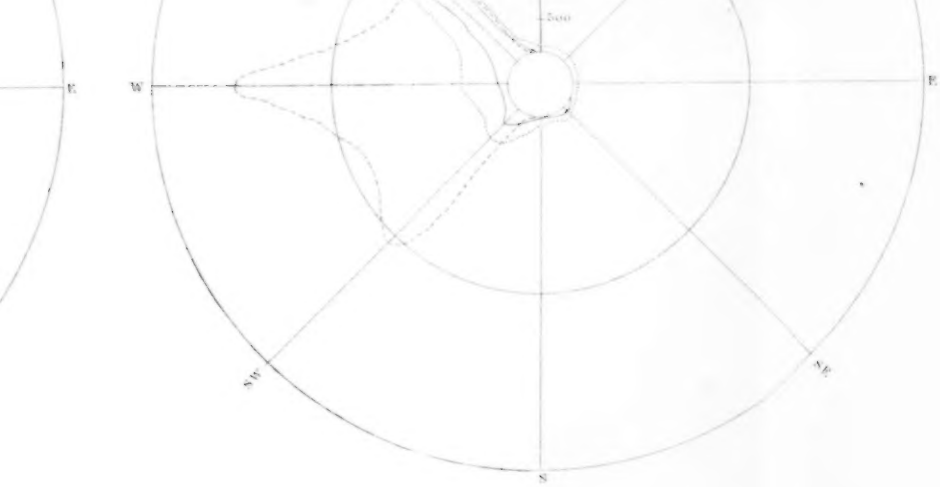


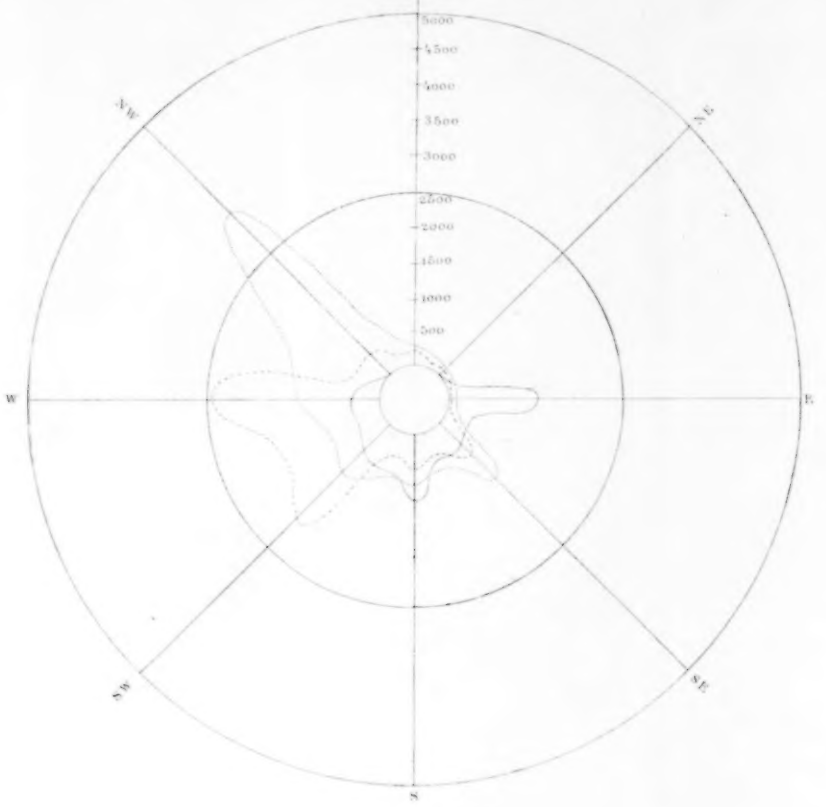
No. 9. SEPTEMBER

No. 13. WHOLE YEAR

No. 14. NOVEMBER to MARCH

No. 2. FEBRUARY

No. 3. MARCH



No. 7. JULY

No. 8. AUGUST

No. 12. DECEMBER

No. 13. WHOLE YEAR

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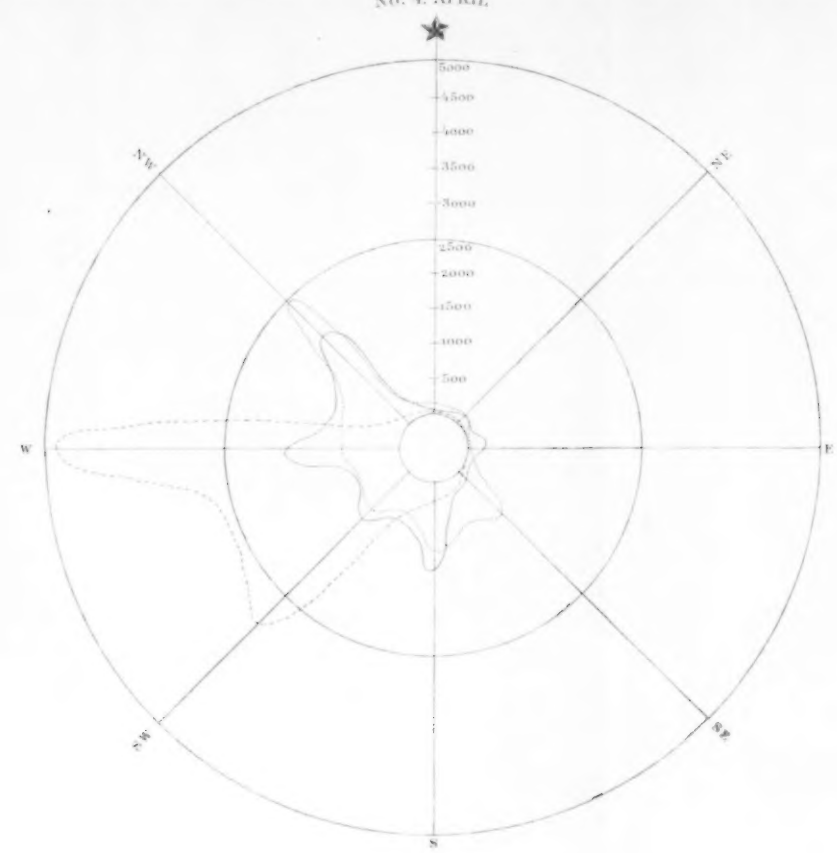
No. 4. APRIL



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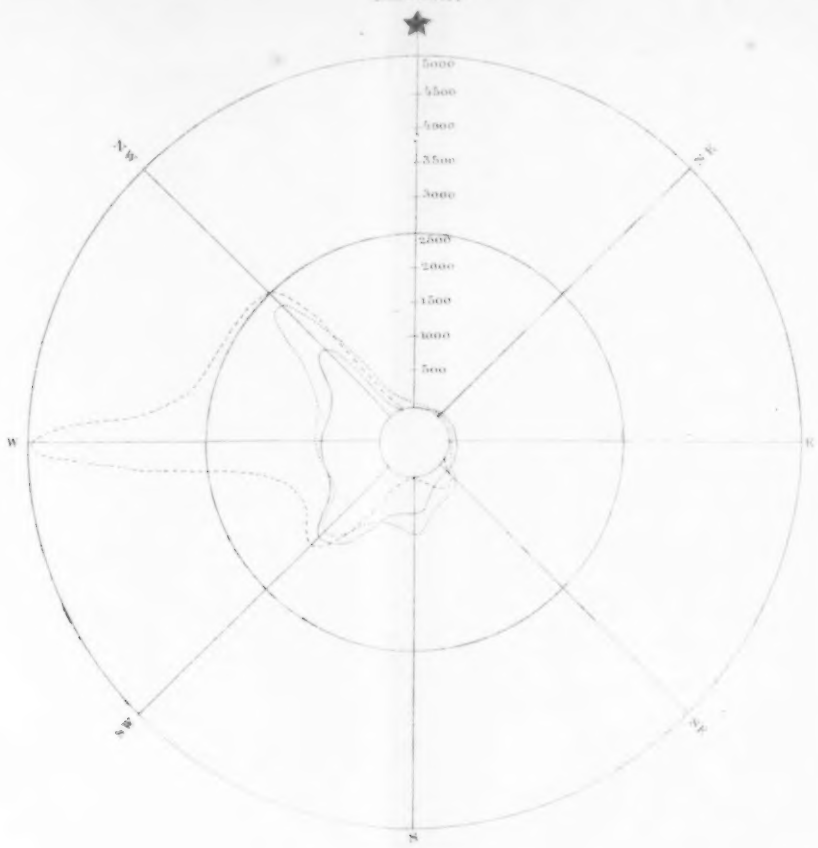
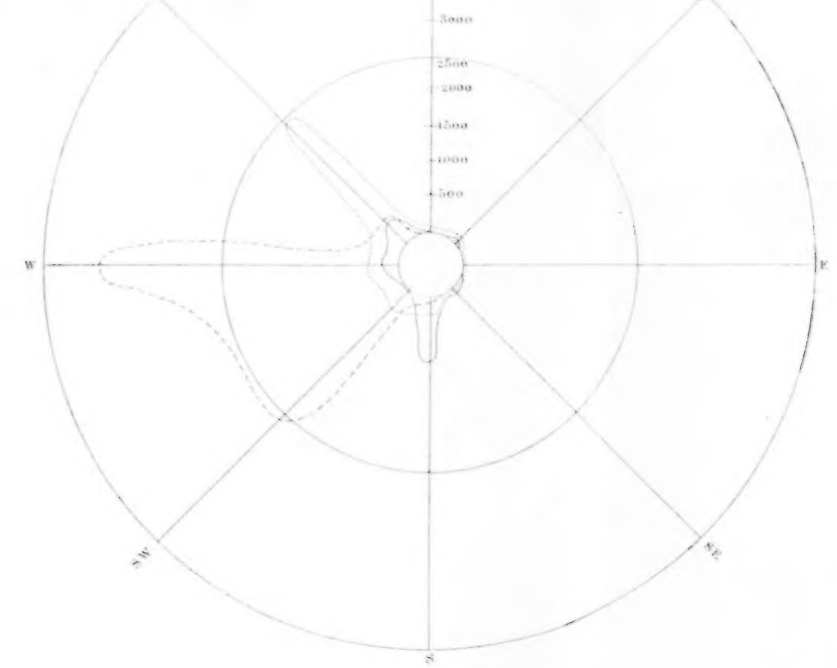
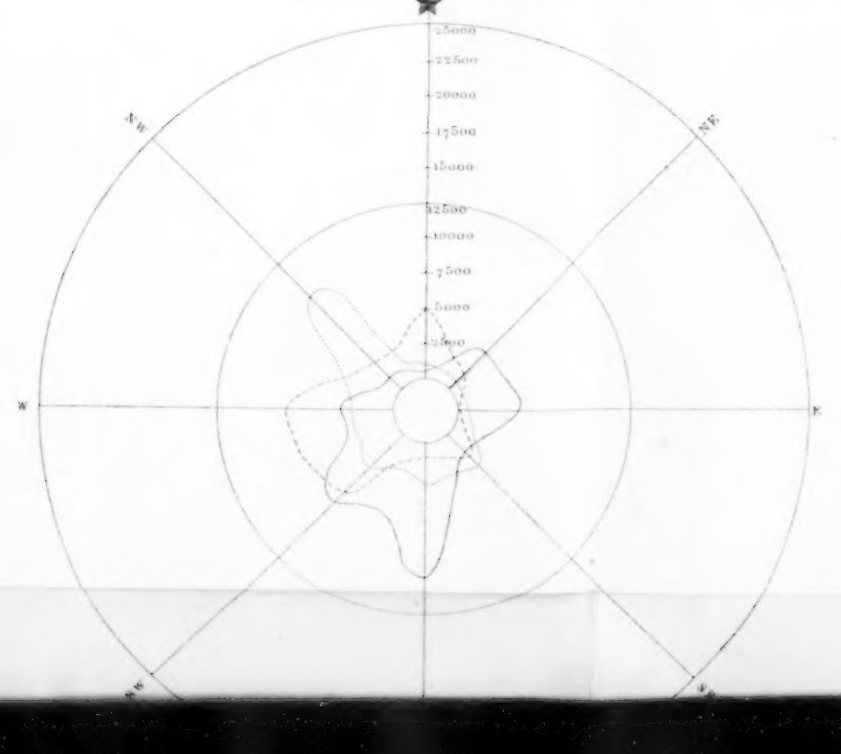


No. 9. SEPTEMBER

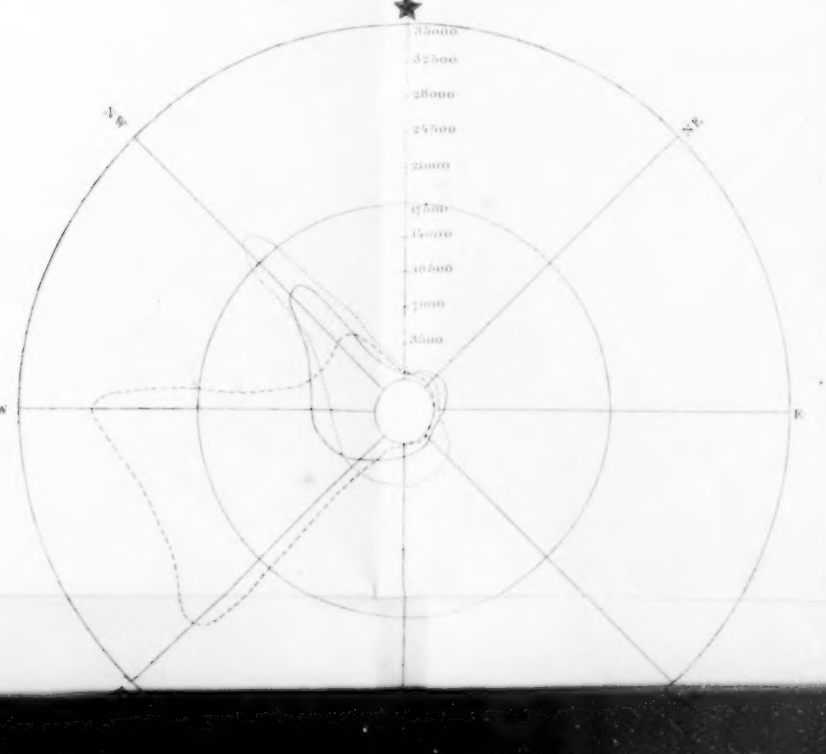


No. 10. OCTOBER

No. 14. NOVEMBER to MARCH



No. 15. APRIL to OCTOBER



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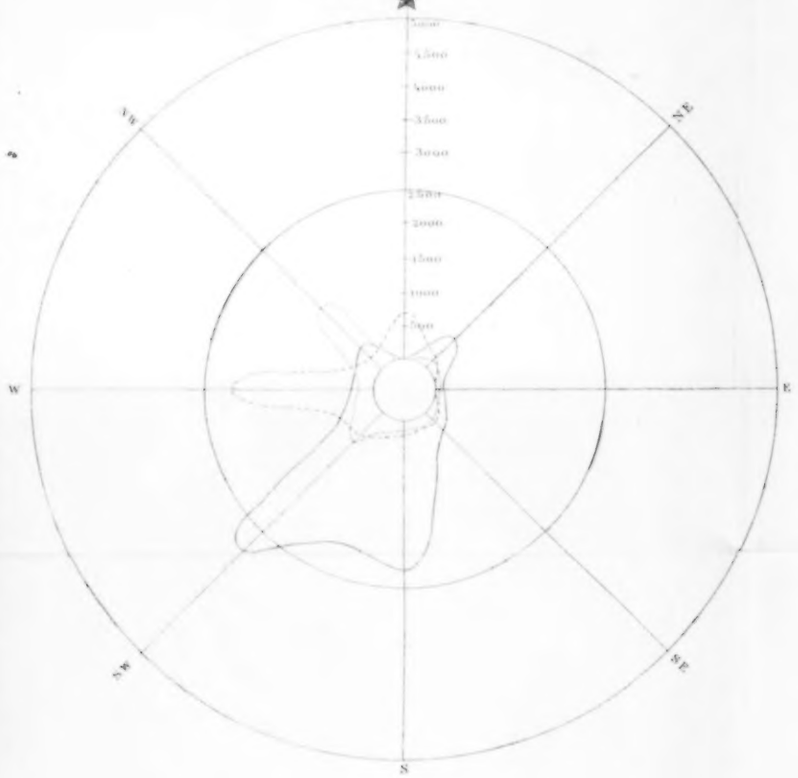
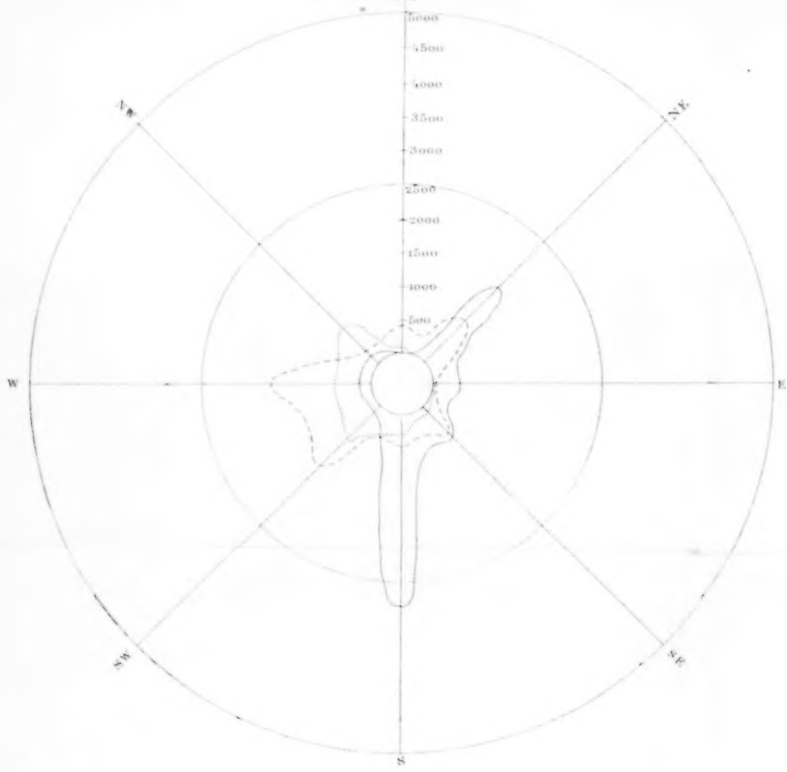
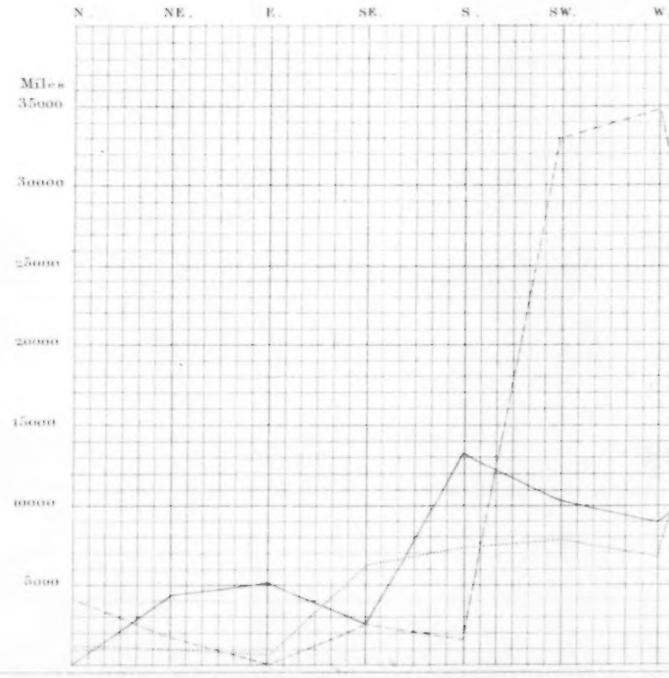


No. 12. DECEMBER



No. 16. Diagram representing the total quantity of wind from each direction during the year



Notes.

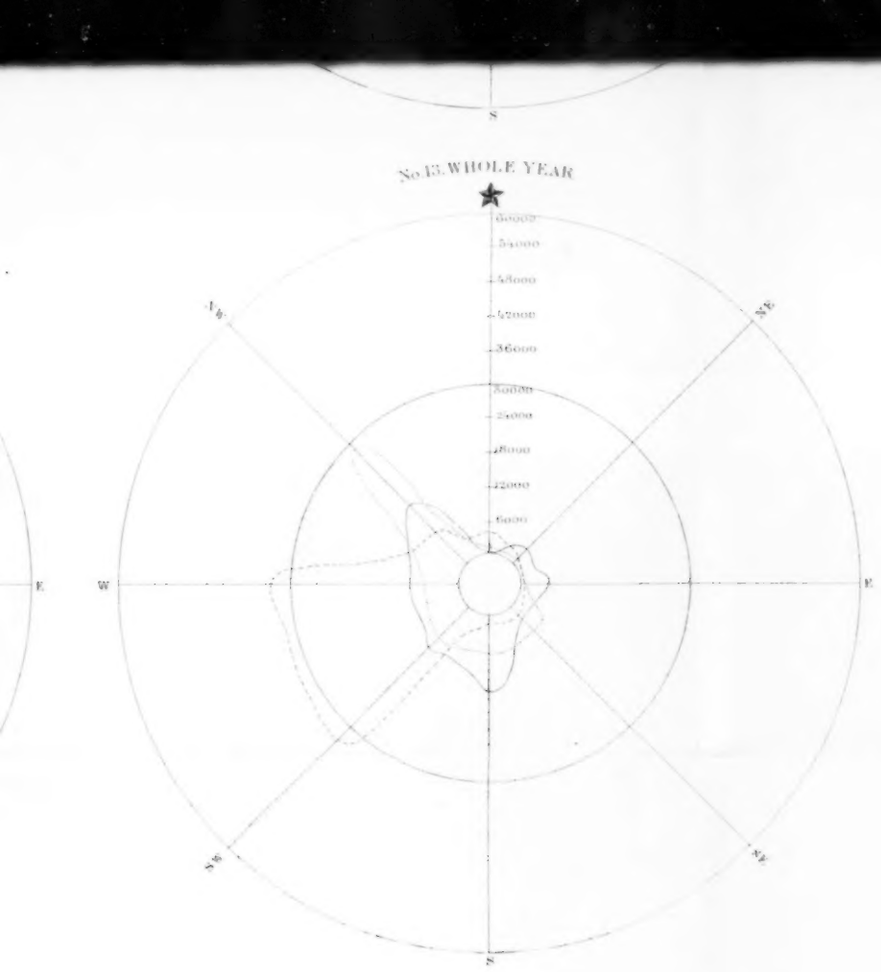
The diagrams plotted on the Compass rose represent the quantity of wind from each direction in every month on a scale of 2500 miles to the inch; for the whole year of 30000 miles to the inch; for the five months from November to March of 12500 miles to the inch; and for the seven months from April to October of 17500 miles to the inch. The circumference of the small circle drawn around the centre of the Compass rose represents the zero or starting point. The observations were made during the year 1855.

The curve for San Diego is represented thus .....

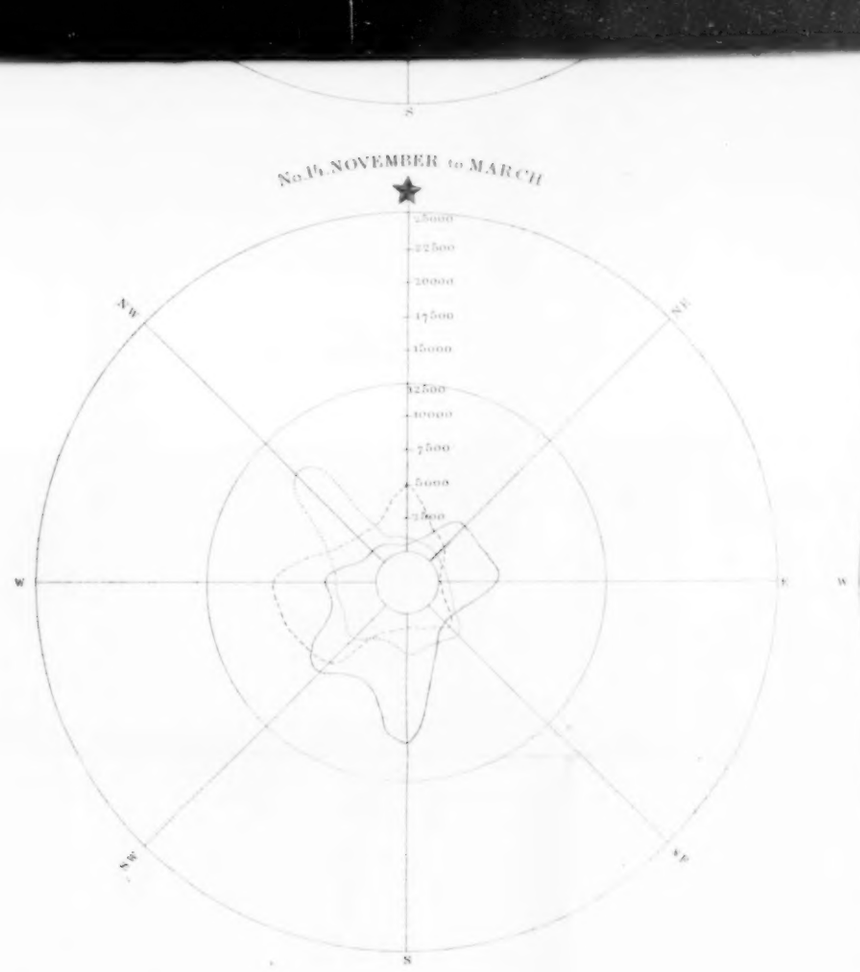
do do do San Francisco do do ———

do do do Astoria do do ———

No 15. WHOLE YEAR



No 16. NOVEMBER to MARCH



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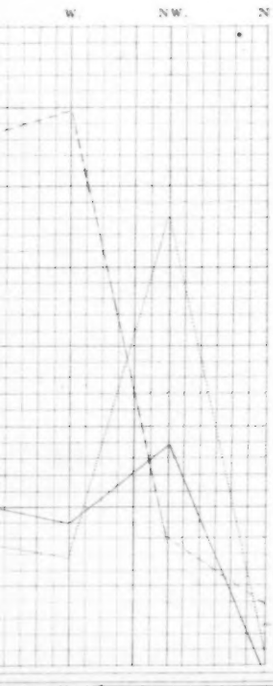
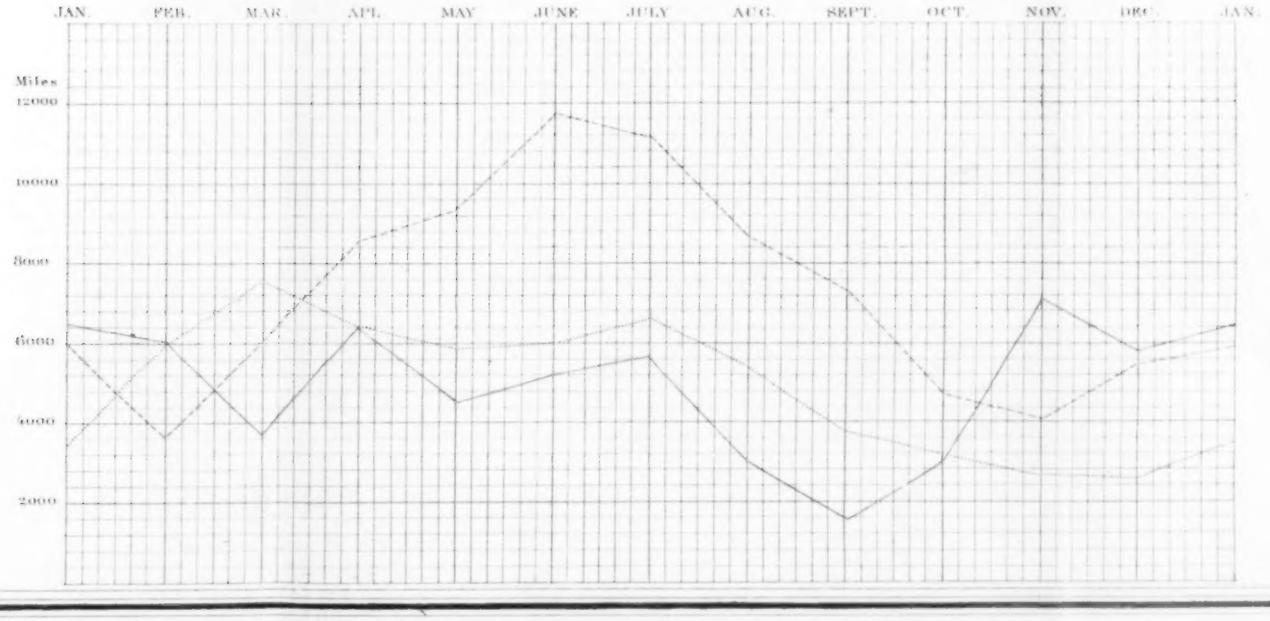
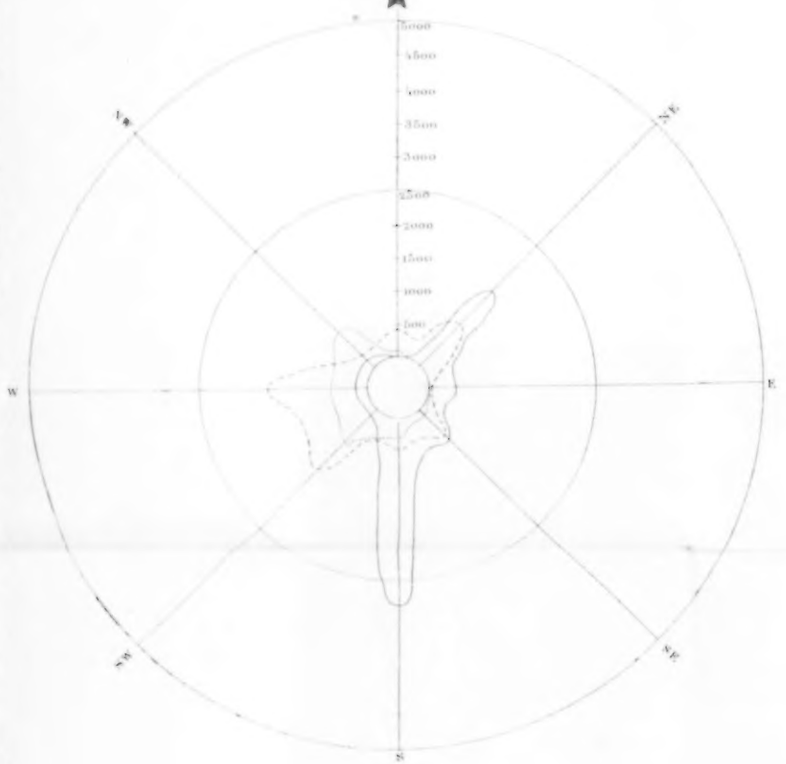


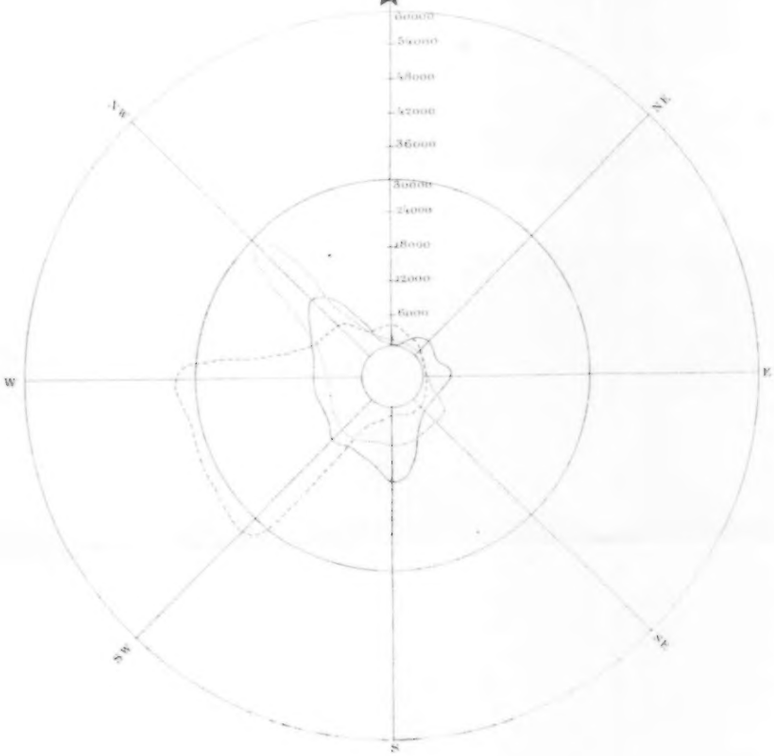
No 17 Diagram representing the total quantity of wind from all directions for each month of the year



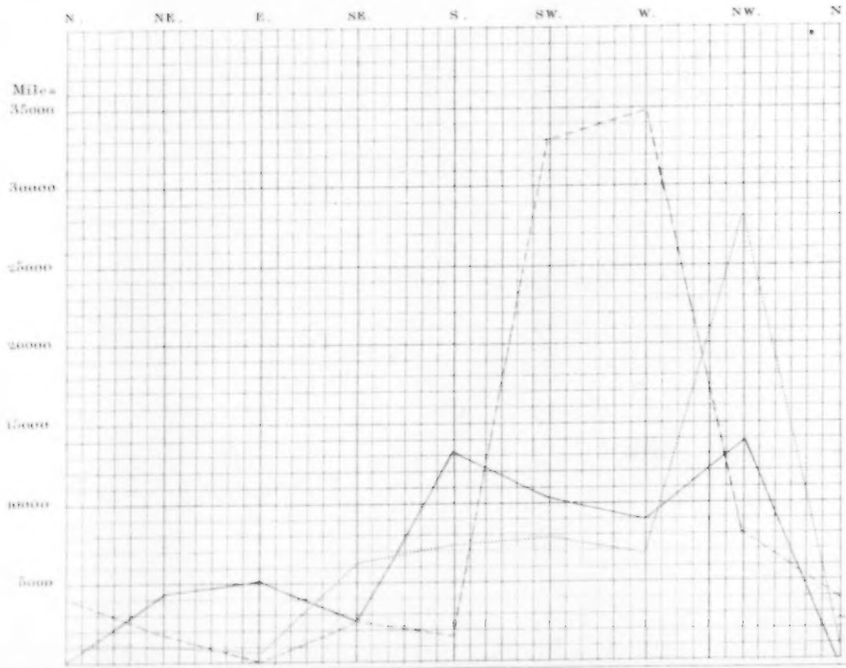
No 12. DECEMBER



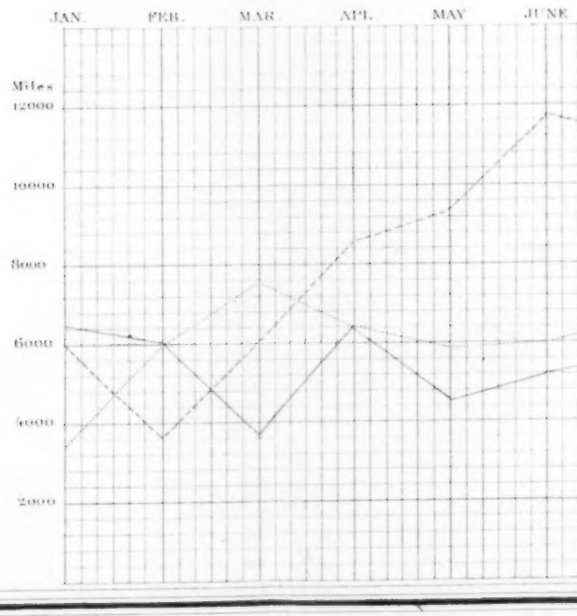
No 13. WHOLE YEAR

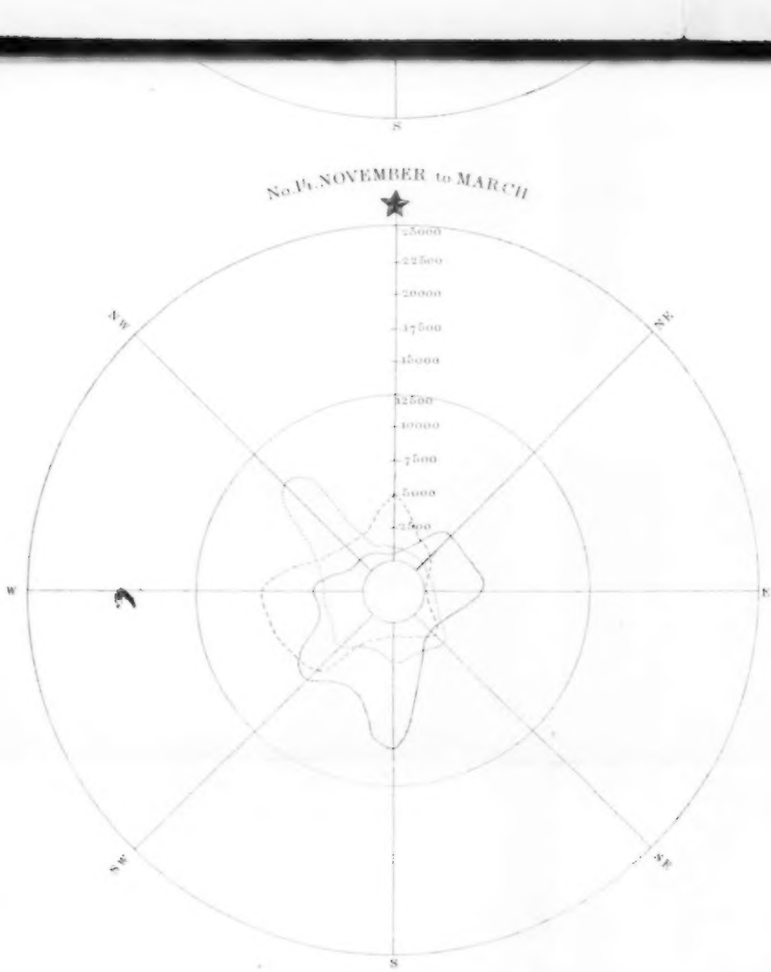


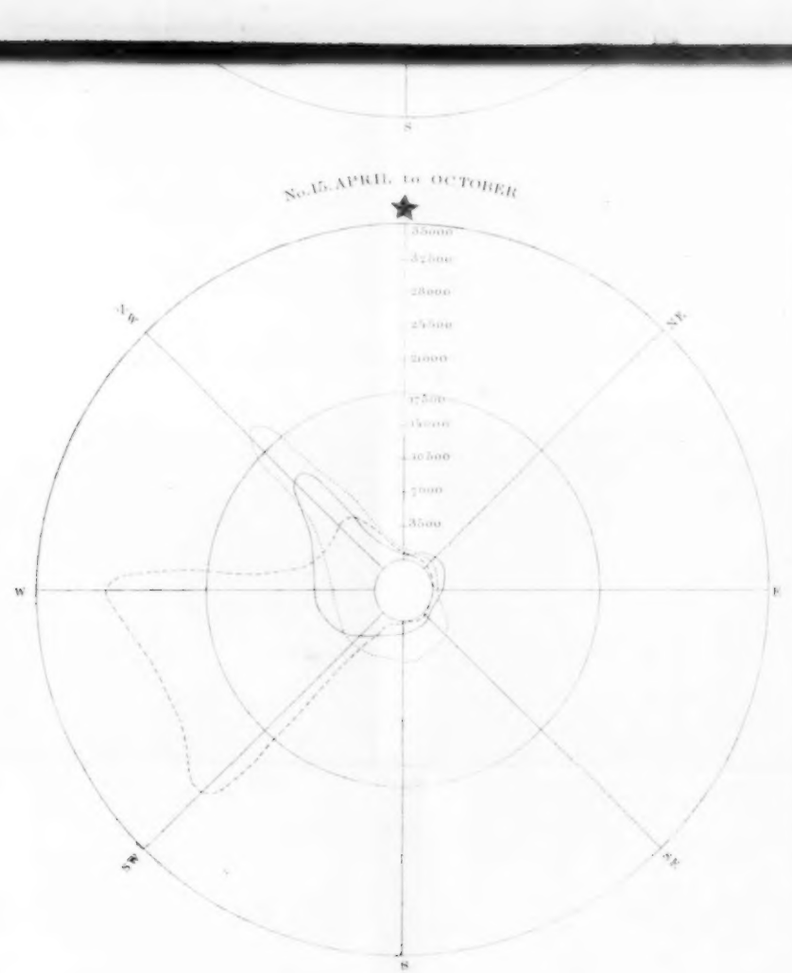
No 16. Diagram representing the total quantity of wind from each direction during the year



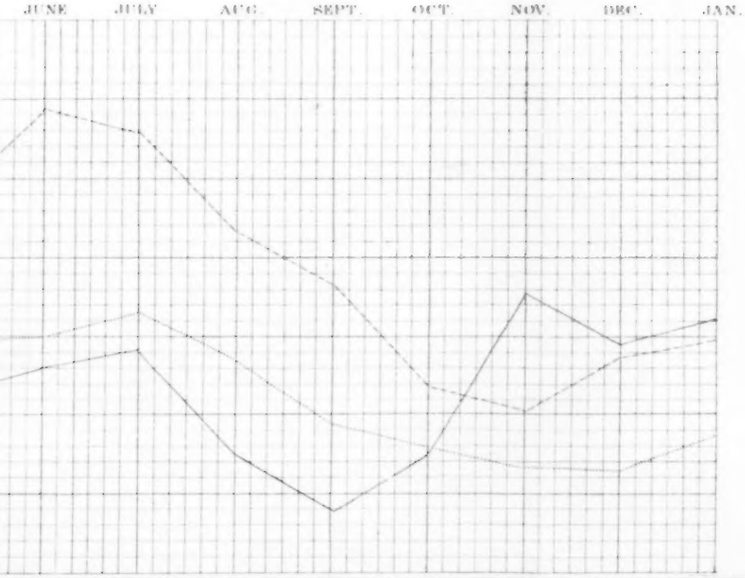
No 17. Diagram representing the total quan







otal quantity of wind from all directions for each month of the year



U.S.COAST SURVEY

A.D.BACHE Supdt.

DIAGRAMS OF WINDS

AT

SAN DIEGO AND SAN FRANCISCO CALIFORNIA

AND ASTORIA OREGON.

From Observations by the U.S.Coast Survey

1857

Sub-assistant J. A. Sullivan and Mr. Webber, and the profile was studied upon a sketch of the plain made by Lieut. Clark.

In 1856 I examined the site, and took steps to obtain the necessary estimates of the cost of preparing it for measurement. The profile of the line as graded gives a good general idea of the ground as it varied but little from the natural profile. (See sketch.)

The whole length of the line is about 8719 meters or 5·4 miles. Its general direction is E. 16° S. (*true bearing.*)

From the eastern end for about four miles the plain is quite level, rising in the first mile pretty regularly about fifteen feet, descending nearly as much in the next to rise by the same quantity in the third mile. It then runs along an elevated level for a fourth of a mile and descends gradually to the rougher part of the base which is included between the 3¾ miles from the east end and the western end of the base.

This line was skillfully graded by Mr. Boutelle, so as to follow the natural surface when the grade did not run above three degrees, and to give as long slopes as possible of the same grade for the convenience of measuring. (See sketch.)

As it was found more economical to make the temporary embankments than to excavate, a profile giving a considerable excess of embankment was selected.

This was executed in the cheapest way which would give stability for the time during which it was required to stand. The least width was twelve feet, of which nine feet was on the south and three feet on the north side of the line to be measured. The base was very carefully aligned. High signals were placed over the termini which are inter-visible. On the Schoodiac a signal of moderate elevation is visible from both, and the distances between this point and the termini were gradually subdivided, until the smallest limit, the distance easily reached by a small transit, was obtained.

The verification of the alignment at different points of the measurement when the seeing was good was complete.

In all these preliminary operations Mr. Boutelle was assisted by Sub-assistant J. A. Sullivan and Mr. Webber.

His grading partly consisted of the farmers and lumbermen of the district who served with great cheerfulness and skill in the use of the heavy implements for rough grading. One of the greatest difficulties was the removal of such boulders as were in the line, many of them being of such size as to require blasting to break them up, and some being actually removed to the required distance from the line by heavy blasts.

The signals erected at the two ends are very substantial, each forty-three feet in height to the top of the tripod and fifty-three to the cone which surmounts them.

The base apparatus already described before the Association, and described and figured in my report for 1854 by Lieut. E. B. Hunt of the Corps of Engineers, was used in this measurement, preliminary trials being made in the office to test its steadiness under the greatest inclination to which it would be subjected, and the accuracy of the surface upon which the measuring stem traverses and which determines the length of the apparatus.

I was assisted in the measurements by Assistant G. W. Dean, Prof. Fairman Rogers, who volunteered for the purpose, Subassistants Goodfellow, Stephen Harris and Sullivan, and Mr. Thomas McDonnell, among whom the different operations were divided.

The usual comparisons of the apparatus with the standard six metre bar, were made before and after the measurement to ascertain that no change had taken place in the length from damage by transportation, and to add to the results of former comparisons.

The measurement was begun at the west end of the line on Saturday the 18th of July, but the next week proved so rainy that it was only resumed in earnest on Monday the 27th.

The work of the first Saturday (24 tubes) was remeasured on the following Monday with precisely the same result as to length, the end of the second measurement falling on the marks which had been placed as terminating the first, and which were fine dots upon the head of a copper nail, placed in a stake some eighteen inches in length driven into the ground until its head just projected above the surface. The position of the mark was determined and verified, as all others of the sort in our measurements, by using a transit placed at right angles to the line and at a moderate distance from it. This was on a descending slope of the strongest grade adopted and there was a difference of temperature of some five degrees in the two measurements.

On Tuesday a length of eighteen tubes, which had been measured on Monday was remeasured with an identical result. This was on an ascending slope. On Monday the work was in part interrupted by the arrangements for photographing the apparatus, on Tuesday by a fog, and on Wednesday by showers in the beginning of the day: we made however half a mile on both days.

On Wednesday began a series of four unbroken days, during the first of which we measured about seven-eighths of a mile, and on the three others a mile, or more than a mile, each day, reaching the east end of the base on Monday evening. Thus counting in the broken days, 5·4 miles were measured in eight days.

This time included the marking of five permanent points near to the ends of the successive miles, where stone posts have since been placed. The ends of the base will be marked by regular monuments. The base of the monument at the west end is cut from the ledge of rocks upon which the signal stands.

By the kindness of Prof. Fairman Rogers I have been enabled to collect approximately some of the statistics of the measurement in a tabular form (No. I). A second table contains the comparison with the other five Coast Survey bases which I have measured.

EPPING BASE.—TABLE I.

Whole length of base in tubes, - - -	1453
" " " " " metres, - - -	8718^m
1^m4245 added at east base making - - or 28,607 feet, or about 5·4 miles.	8719^m4245
Diff. of level between highest and lowest points, -	104 ft. nearly.
Mean level of Base above mean tide, - -	257 ft. or 78^m43
Approx. corr. for reduction to the level of the sea, - or 4 inches nearly.	$0^m·10714422$
No. of tubes inclined, - - - - -	643
" " " level, - - - - - -	810
Ratio of tubes inclined to whole number, -	0·442 nearly.
" " " level " " " - -	0·550
Correction for versed sine for whole base, - or 9·2 ft. to be subtracted.	$2^m·8038437$

Maximum inclination,				8° 14′		Ratio to whole number inclined.
Number of tubes inclined,				3° and over	31	0·048
"	"	"	"	2 30′ " "	234	0·364
"	"	"	"	2 " "	79	0·123
"	"	"	"	1 30 " "	120	0·186
"	"	"	"	1 " "	110	0·171
"	"	"	"	0 30 " "	21	0·032
"	"	"	"		48	0·074
					643	

Greatest day's work 281 tubes 1·05 mile in $11^h\ 10^m$ working time.
Averaging 1 tube in $2^m\ 27^s$.
Greatest number in one hour 37, or $1^m\ 37^s$ for each tube.

TABLE II.

Comparative Table of the Measurements of six U. S. Coast Survey Bases.

	Dauphine Island.	Bodies Island.	Edisto Island.	Key Biscayne	Cape Sable.	Epping Plains.
Whole No. of tubes measured,	1777	1807	1787	965	1072	1453
" " days employed,	17	10	13	9	8	8
" " hours "	$143^h\ 17^m$	$81^h\ 08^m$	$97^h\ 28^m$	$66^h\ 31^m$	$46^h\ 26^m$	$69^h\ 43^m$
" " tubes level,	961	1496	862	473	994	810
" " " inclined,	816	311	925	492	78	643
Ave'ge length of working day,	$8^h\ 25^m\ 7^s$	$8^h\ 07^m$	$7^h\ 30^m$	$7^h\ 23^m$	$5^h\ 48^m\ 18^s$	$10^h\ 07^m$
" time of one tube,	$5^m\ 32^s$	$2^m\ 54^s$	$3^m\ 22^s$	$4^m\ 20^s$	$2^m\ 51^s$	$2^m\ 58^s$
" No. of tubes per day,	104·5	180·7	137·5	107·2	134	181·6
" No. of tubes pr. d'y of 9^h,	108·0	197·9	165·9	130·0	200·0	187·2
" " " " " hour,	11·85	21·98	18·40	14·40	22·47	20·8
" plus inclination,	17′·6	16′·1	24′·5	31′·0	12′·0	1° 53′
" minus "	16′·6	19′·6	23′·0	26′·0	10′·0	1° 54′
" of greatest plus inclin.	40′·8	23′·7	55′·4	58′·0	14′·0	2° 52′
" " " minus "	42′·6	29′·5	48′·4	54′·0	11′·0	2° 46′
" temperature Fahr.	84° 5	52° 0	59° 5	82° 9	87° 9	70° 0′

The photographs of the apparatus and operations which I submit to the Association, were taken by Mr. Black, of the firm of Whipple and Black, Boston, who exerted himself especially in the matter and succeeded, under many disadvantages, from variable weather and the roughness of field arrangements for photography, in making satisfactory representations.

The views of the apparatus and operations (see Plate) include the placing of the apparatus over a mark, the aligning, the setting of the trestles in advance of the measurement, the transfer of the measuring tube and the making of contact.

The comparing apparatus and tent are also shown (see Plate lettered No. 1, Plans, Sections and Profile). One of the sketches shows the topographical features of the ground, and another gives the profile of the base as graded for measurement.

ART. IX.—*On the Influence of Musical Sounds on the Flame of a Jet of Coal-gas;* by JOHN LECONTE, M.D., Professor of Natural Philosophy in the South Carolina College.

A SHORT time after reading Prof. John Tyndall's excellent article "On the Sounds produced by the Combustion of Gases in Tubes,"* I happened to be one of a party of eight persons assembled after tea for the purpose of enjoying a private musical entertainment. Three instruments were employed in the performance of several of the grand trios of Beethoven, namely, the piano, violin, and violoncello. Two "*fish-tail*" gas-burners projected from the brick wall near the piano. Both of them burnt with remarkable steadiness, the windows being closed and the air of the room being very calm. Nevertheless it was evident, that *one* of them was under a pressure nearly sufficient to make it *flare*.

Soon after the music commenced, I observed that the flame of the last-mentioned burner exhibited pulsations in height, which were *exactly synchronous* with the audible beats. This phenomenon was very striking to every one in the room, and especially so when the strong notes of the violoncello came in. It was exceedingly interesting to observe how perfectly even the *trills* of this instrument were reflected on the sheet of flame. *A deaf man might have seen the harmony.* As the evening advanced, and the diminished consumption of gas in the city *increased the pressure*, the phenomenon became more conspicuous. The *jumping* of the flame gradually increased—became somewhat irregular—and finally it began to flare continuously, emitting the characteristic sound indicating the escape of a greater amount of gas than

* *Vide* Philosophical Magazine, 4th Series, vol. xiii, p. 473, 1857.

could be properly consumed. I then ascertained by experiment, that the phenomenon *did not* take place unless the discharge of gas was so regulated that the flame approximated the condition of *flaring*. I likewise determined by experiment, that the effects *were not* produced by jarring or shaking the floor and walls of the room by means of repeated concussions. Hence it is obvious, that the pulsations of the flame *were not* owing to *indirect* vibrations propagated through the medium of the walls of the room to the burning apparatus, but must have been produced by the *direct* influence of the ærial sonorous pulses on the burning jet.

In the experiments of M. Schaffgotsch and Prof. J. Tyndall, it is evident that "the shaking of the singing flame within the glass tube," produced by the voice or the syrene, was a phenomenon perfectly analogous to what took place under my observation *without the intervention of a tube*. In my case, the discharge of gas was so regulated that there was a tendency in the flame to flare, or to emit a "*singing sound*." Under these circumstances, strong aerial pulsations occurring at *regular intervals*, were sufficient to develop synchronous fluctuations in the height of the flame. It is probable that the effects would be more striking when the tones of the musical instrument are *nearly* in unison with the sounds which would be produced by the flame under the slight increase in the rapidity of discharge of gas required to manifest the phenomenon of flaring. This point might be submitted to an experimental test.

As in Prof. Tyndall's experiments on the jet of gas burning within a tube, clapping of the hands, shouting, etc. were ineffectual in converting the "silent" into the "singing flame,"—so, in the case under consideration, *irregular* sounds did not produce any perceptible influence. It seems to be necessary that the impulses should *accumulate* in order to exercise an appreciable effect.

With regard to the mode in which the sounds are produced by the combustion of gases in tubes, it is universally admitted, that the explanation given by Prof. Faraday in 1818 is essentially correct. It is well known that he referred these sounds to the successive explosions produced by the periodic combination of the atmospheric oxygen with the issuing jet of gas. While reading Prof. J. Plateau's admirable researches (third series) on the "Theory of the modifications experienced by Jets of Liquid issuing from circular orifices when exposed to the influence of Vibratory Motions,"* the idea flashed across my mind that the phenomenon which had fallen under my observation, was nothing more than a *particular case* of the effects of sounds on *all kinds*

* Philosophical Magazine, 4th series, vol. xiv, p. 1 et seq., July, 1857.

of fluid jets. Subsequent reflection has only served to fortify this first impression.

The beautiful investigations of Felix Savart on the influence of sounds on jets of water, afford results presenting so many points of analogy with their effects on the jet of burning gas, that it may be well to inquire whether both of them may be referred to a common cause. In order to place this in a striking light, I shall subjoin some of the results of Savart's experiments. Vertically descending jets of water receive the following modifications under the influence of vibrations:—

1. The continuous portions become shortened; the vein resolves itself into separate drops nearer the orifice, than when *not* under the influence of vibrations.

2. Each of the masses, as they detach themselves from the extremity of the continuous part, becomes flattened alternately in a vertical and horizontal direction, presenting to the eye, under the influence of their translatory motion, regularly disposed series of maxima and minima of thickness, or ventral segments and nodes.

3. The foregoing modifications become much more developed and regular, when a note, in unison with that which would be produced by the shock of the discontinuous part of the jet against a stretched membrane, is sounded in its neighborhood. The continuous part becomes considerably shortened, and the ventral segments are enlarged.

4. When the note of the instrument is *almost* in unison, the continuous part of the jet is alternately lengthened and shortened, and the beats which coincide with these variations in length *can be recognized by the ear.*

5. Other tones act with less energy on the jet, and some produce no sensible effect.

When a jet is made to ascend *obliquely*, so that the discontinuous part appears scattered into a kind of *sheaf* in the same vertical plane, M. Savart found:—

a. That under the influence of vibrations of a determinate period, this sheaf may form itself into *two* distinct jets, each possessing regularly disposed ventral segments and nodes; sometimes, with a different note, the sheaf becomes replaced by *three* jets.

b. The note which produces the greatest shortening of the continuous part, always reduces the whole to a *single jet*, presenting a perfectly regular system of ventral segments and nodes.

In the last memoir of M. Savart—a posthumous one—presented to the Academy of Sciences of Paris by M. Arago in 1853,* several remarkable acoustic phenomena are noticed in

* Comptes Rendus for August 1853. Also Phil. Mag., 4th series, vol. vii, p. 186, 1854.

relation to the musical tones produced by the efflux of liquids through short tubes. When certain precautions and conditions are observed (which are minutely detailed by this able experimentalist), the discharge of the liquid gives rise to a succession of musical tones of great intensity and of a peculiar quality, somewhat analogous to that of the human voice. That these notes were not produced by the descending drops of the liquid vein, was proved by permitting it to discharge itself into a vessel of water, while the orifice was below the surface of the latter. In this case, the jet of liquid must have been *continuous*, but nevertheless the notes were produced. These unexpected results have been entirely confirmed by the more recent experiments of Prof. Tyndall.*

According to the researches of M. Plateau, all of the phenomena of the influence of vibrations on jets of liquid, are referable to the conflict between the vibrations and the *forces of figure* ("*forces figuratrices*"). If the physical fact is admitted,—and it seems to be indisputable,—that a liquid cylinder attains a *limit of stability* when the proportion between its length and its diameter is in the ratio of twenty-two to seven, it is almost a *physical necessity* that the jet should assume the constitution indicated by the observations of Savart. It likewise seems highly probable that a liquid jet, while in a transition stage to discontinuous drops, should be exceedingly sensitive to the influence of all kinds of vibrations. It must be confessed, however, that Plateau's beautiful and coherent theory does not appear to embrace Savart's last experiment, in which the musical tones were produced by a jet of water issuing under the surface of the same liquid. It is rather difficult to imagine what agency the "forces of figure" could have, under such circumstances, in the production of the phenomenon. This curious experiment tends to corroborate Savart's original idea, that the vibrations which produce the sounds must take place in the glass reservoir itself, and that the cause must be inherent in the phenomenon of the flow.

To apply the principles of Plateau's theory to gaseous jets, we are compelled to abandon the idea of the *non-existence of molecular cohesion in gases*. But is there not abundant evidence to show that cohesion *does exist* among the particles of gaseous masses? Does not the deviation from rigorous accuracy both in the law of Mariotte and of Gay-Lussac,—especially in the case of condensable gases, as shown by the admirable experiments of M. Regnault,—clearly prove, that the hypothesis of the non-existence of cohesion in aeriform bodies is fallacious? Do not the expanding rings which ascend when a bubble of phosphu-

* Philosophical Magazine, 4th series, vol. viii, p. 74, 1854.

retted hydrogen takes fire in the air, indicate the existence of some cohesive force in the gaseous product of combustion (aqueous vapor), whose outlines are marked by the opake phosphoric acid? In short, does not the very *form* of the flame of a "fish-tail" burner demonstrate that cohesion *must exist* among the particles of the issuing gas? It is well known that, in this burner, the single jet which issues is formed by the union of *two oblique jets* immediately before the gas is emitted. The result is a perpendicular *sheet of flame*. How is such a result produced by the mutual action of two jets, unless the force of cohesion is brought into play? Is it not obvious, that such a fan-like flame must be produced by the same causes as those varied and beautiful forms of aqueous sheets developed by the mutual action of jets of water, so strikingly exhibited in the experiments of Savart and of Magnus?

If it be granted that gases possess molecular cohesion, it seems to be physically certain, that jets of gas must be subject to the same laws as those of liquid. Vibratory movements excited in the neighborhood, ought, therefore, to produce modifications in them analogous to those recorded by M. Savart in relation to jets of water. Flame or incandescent gas presents gaseous matter in a *visible* form, admirably adapted for experimental investigation; and *when produced by a jet*, should be amenable to the principles of Plateau's theory. According to this view, the pulsations or *beats* which I observed in the gas-flame when under the influence of musical sounds, are produced by the conflict between the aerial vibrations and the "forces of figure" (as Plateau calls them), giving origin to periodical fluctuations of intensity, depending on the sonorous pulses.

If this view is correct, will it not be necessary for us to modify our ideas in relation to the agency of tubes in developing musical sounds by means of burning jets of gas? Must we not look upon all burning jets,—as in the case of water-jets,—as *musically inclined;* and that the use of tubes merely places them in a condition favorable for developing the tones? It is well known, that burning jets frequently emit a *singing sound* when they are perfectly *free*. Are these sounds produced by successive explosions analogous to those which take place in glass tubes? It is very certain, that under the influence of molecular forces, any cause which tends to elongate the flame, without affecting the velocity of discharge, must tend to render it discontinuous, and thus bring about that mixture of gas and air which is essential to the production of the explosions. The influence of tubes as well as of aerial vibrations in establishing this condition of things is sufficiently obvious. Was not the "beaded line" with its succession of "luminous stars," which Prof. Tyndall observed when a flame of olefiant gas burning in a tube, was examined by means

of a moving mirror, an indication that the flame became *discontinuous*, precisely as the continuous part of a jet of water becomes *shortened*, and resolved into isolated drops, under the influence of sonorous pulsations? But I forbear enlarging on this very interesting subject, inasmuch as the accomplished physicist last named, has promised to examine it at a future period. In the hands of so sagacious a philosopher, we may anticipate a most searching investigation of the phenomena in all their relations. In the mean time, I wish to call the attention of men of science to the view presented in this article, in so far as it groups together several classes of phenomena under one head, and may be considered a partial generalization.

Columbia, South Carolina, Oct., 1857.

ART. X.—*On the Motion of the Gyroscope as modified by the retarding forces of friction and the resistance of the air: with a brief analysis of the "Top;"* by Maj. J. G. BARNARD, A. M., Corps of Engineers, U. S. A.

IN a previous paper (see article in this Journal for July, 1857, to which this paper is intended to be supplementary,) I have investigated the "Self-sustaining power of the Gyroscope" in the light of analysis. From the general equations of "Rotary motion" I have deduced the laws of motion for the particular case of a *solid of revolution* moving about a fixed point in its axis of figure, (or the prolongation thereof). I have shown that such a body, having its axis placed in any degree of inclination to the vertical, and having a high rotary motion about *that axis*, will not, under the influence of gravity, *sensibly fall;* but that any point in the axis will describe "an undulating curve whose superior culminations are *cusps* lying in the same horizontal plane;" that this curve approaches more and more nearly to the cycloid, as the velocity of axial rotation is greater; that when this velocity is very great the undulations become very minute and "the axis of figure performing undulations too rapid and too minute to be perceived, moves slowly about its point of support." I have shown how the direction and velocity of this *gyration* are determined by the direction and velocity of axial rotation and the distance of the center of gravity of the figure from the point of support, and that the remarkable phenomenon exhibited by the gyroscope is but a *particular case* due to a *very high velocity* of axial rotation, of the general laws of motion of such a body as described, which embrace the motion of the pendulum in one extreme and that of the gyroscope in the other, and that intermediate between

these two extreme cases (for moderate rotary velocities) the undulations of the axis, will be large and sensible.

I have likewise shown that whenever, to the axis of a rotating solid, an angular velocity is imparted, a force which I have called "*the deflecting force*" acting perpendicular to the plane of motion of that axis, is developed, whose intensity is proportional to this angular velocity, and likewise to the rotary velocity of the body; and that it is this *deflecting force* which is the immediate *sustaining agent*, in the gyroscope.

In the above deductions of analysis is found the full and complete solution of the "self-sustaining power of the gyroscope."

To make the character of the motion indicated by analysis, sensible to the eye, it is only necessary to attach to the ordinary gyroscope, in the prolongation of the axis, an arm of five or six inches in length, and having an universal joint at its extremity, and to swing the instrument as a pendulum; or, the extremity of an arm of such a length may be rested in the usual way, upon the point of the standard, when, with the centre of gyration removed at so great a distance from the point of support, the undulatory motion becomes very evident.

But it cannot fail to be observed that the motion preserves this peculiar feature but for a very short period. The undulations speedily disappear; instead of periodical moments of *rest* (which the theory requires at each *cusp*) the gyratory velocity becomes *continuous*, and nearly uniform and horizontal; and it increases as the axis (owing to the retarding influences of friction and the resistance of the air) slowly falls. In short, the axis soon seems to move upon a descending spiral described about a vertical through the point of support.

The experimental gyroscope, in its simplest form consists of two distinct masses, the rotating disk, and the *mounting* (or ring in which the disk turns). The point of support in the latter, though it gives free motion about a vertical axis, constrains more or less, the motion of the combined mass about any other. The rotating disk turns at the extremities of its axle, upon points or surfaces in the mass of the mounting, *with friction;* it is rare, too, that the point of support, of the mounting, is adjusted in the exact prolongation of the axis of the disk.

Without attempting to subject to analysis causes so difficult to grasp as these, I shall first attempt to show, by general considerations, what would be the *immediate* influence of the retarding forces of friction and the resistance of the air upon our theoretical solid; and then point out the further effect due to the discrepancies of figure, above indicated. Leaving out of consideration the minute effect of friction at the point of support, these forces exert their influence, mainly in retarding the *rotary velocity of the disk.* Friction—at the extremities of the axle of

the disk, and the resistance of the air, at its surface, are powerful enough to destroy entirely in a very few minutes, the high velocity originally given to it. It is in this way, mainly, that they modify the motion indicated by analysis.

If the rotary velocity remained *constant* while the axis made *one* of the little cycloidal curves $a\,b\,a'$, (fig. 1) the deflecting force would be just sufficient, as I have shown (p. 68 of the article cited) to lift the axis back to its original elevation a', and to destroy, *entirely*, the velocity it had acquired through its fall $c\,b$. If, at a', the rotary velocity n underwent an *instantaneous* diminution, and remained constant through another undulation, a curve, of larger amplitude and sagitta $a'\,b'\,a''$ would be described, and the axis would *again* rise to its original elevation a'', and *again* be brought to rest. We might then, on casual consideration of the subject, expect to see the undulations become more and more sensible as the rotary velocity decreased. The reverse, however, is the case, as I have already stated. In fact, the above supposition would require the rotary velocity n to be a *discontinuous* decreasing function of the time; whereas it is, really a *continuous* decreasing function. It is undergoing a gradual diminution between a and a'. The *deflecting force*, which is constantly proportional to it, is *therefore* insufficient to keep the axis up to the theoretical curve $a\,b\,a'$, but a *lower* curve $a\,b_1\,a_1$ is described; and when the culmination a_1 is reached, it is *below* the original elevation a'.

1.

But the 2d of our general equations for the gyroscope (4), [afterwards put under the simple form $\left(\text{eq. } (f)\right) v_s^2 = \frac{2g}{\gamma} h$] which is *independent of* n, shows that the angular velocity of the axis will always be that due to its *actual fall* h below the initial elevation. On reaching the culmination a_1 therefore, the axis will not come to rest, but will have a horizontal velocity due to the fall $a'\,a_1$ and the curve will not form a *cusp* but an *inflexion* at a_1.

The axis will commence its second descent, therefore, with an *initial horizontal velocity*. It will not descend as much as it would have

done had it started *from rest* with its diminished value of n; and, for the same reason as before, will not be able to rise again as high as its starting point a_1 but to a somewhat lower point a_2 and with an increased horizontal velocity. These increments of horizontal velocity will constantly ensue as the culminations become lower and lower, while on the other hand, the undulations become less and less marked, as indicated by the figure.

I have stated in my former paper (p. 71) that a certain *initial* horizontal angular velocity such as would "make its corresponding deflecting force equal to the component of gravity, $g \sin \theta$, would cause a horizontal motion without undulation." This horizontal velocity is rapidly attained through the agencies just described: or, at least, nearly approximated to, and the axis, as observation shows, soon acquires a continuous and uniform horizontal motion.

On the other hand, this sustaining power being directly proportional to the rotary velocity of the disk, as well as to the angular velocity of the axis, diminishes with the former, and as it diminishes, the axis must descend, acquiring angular velocity due to the height of fall: hence the rapid gyration and the descending spiral motion which accompanies the loss of rotary velocity.

A more curious and puzzling effect of the friction of the axle is presented, when we come to take into consideration, instead of our theoretical solid, the discrepancies of figure presented by the actual gyroscope. If, with a high initial rotation, the common gyroscope be placed on its point of support with its axis somewhat inclined *above* a horizontal position, it will soon be observed to *rise*. In my analytical examination (p. 55) I have stated as a deduction from the second equation (4), that "the axis of figure can never rise *above* its initial angle of elevation." That equation supposes that the rotary velocity n *remains unimpaired*, and is the expression of a fundamental principle of dynamics—that of "living forces" (so-called), which requires that the living force generated by gravity be directly proportional to the height *of fall*, and involves as a corollary that through the agency of its own gravity alone, the centre of gravity of a body can never rise above its initial height.* The anomaly observed, therefore, either requires the action of some *foreign force; or*, that the living force lost by the rotating disk, shall, through some hidden agency, be expended in performing this work of *lifting* the mass.

* The first of these equations (as I have remarked in a note to p. 59) is the expression of another fundamental principle—more usually called the "principle of areas."

The discrepancy here exhibited between the motion proper to our theoretical solid of revolution and the experimental gyroscope is due to the division of the latter into two distinct masses, one of which rotates, *with friction*, upon points or surfaces in the other; and to the fact that at the point of support (in the latter) there is not *perfectly free motion* in all directions.

The friction at the extremities of the axle of the disk, tends to impress on the mass which constitutes the "mounting," a rotation in the same direction. Were the motion of the latter upon its fixed point of support *perfectly free*, the mounting and disk would soon acquire a *common rotatory velocity* about the axis of the disk. But the mounting *is* perfectly free to turn about the *vertical* axis through the point of support, though *not about any other*. If we decompose, therefore, the rotation which would be impressed upon the mounting into two components, one about this vertical, and the other about a horizontal axis—the first takes *full effect*, and the latter is destroyed at the point of support. If the axis of the instrument is *above* the horizontal, this component of rotation is in the same direction as the *gyration* due to gravity, and *adds to it;* if the axis is *below* the horizontal, the component is the reverse of the natural gyration, and *diminishes it*.

But I have shown that the axis soon acquires, independent of this cause, a gyration whose deflecting or sustaining force is just equivalent to the downward component of gravity. The *addition* to this gyratory velocity caused by friction when the axis is inclined *upwards* puts the deflecting force in *excess*, and the axis is raised; it is raised, as in all other cases in which *work* is done through acquired velocity—viz., by an expenditure of *living force;* but in this instance, through a most curious and complicated series of agencies.

The phenomenon may be best illustrated in the following manner. Let the outer extremity of the common gyroscope, having its axis inclined *above* the horizontal, be supported by a thread attached to some fixed point vertically above the point of support, so that gyration shall be free. Here gravity is eliminated, and the axis of our theoretical solid of revolution would remain perfectly motionless; but the gyroscope starts off, of itself, to gyrate in *the same direction* that it would were its extremity *free*. This gyration increases (if the rotary velocity is great) until the deflecting force due to it, lifts the outer extremity from its support on the thread, and it continues indefinitely to rise. Try the same experiment with the axis *below* the horizontal. The gyration will commence spontaneously as before, but in the *reverse* direction: it will increase until the *inner extremity is lifted from the point of support*, (the action of the deflecting force being here reversed,) the instrument supporting itself on the thread

alone. If the experiment is tried with the axis perfectly horizontal, no gyration takes place, for the component of rotation, due to friction, is, in this position, zero.

The foregoing reasoning accounts, I believe, for all the observed phenomena of the experimental gyroscope, and shows how, from the theory of our imaginary solid of revolution, a consideration of the effects of the discrepancies of form, and of the actual disturbing forces, leads to their satisfactory explanation.

The great similarity between the phenomena of the top and gyroscope, renders it not uninteresting to compare the laws of motion of the two. If we conceive a solid of revolution terminated at its lower extremity by a *point* (the ordinary form of the top), resting upon a horizontal plane without friction, *and having a rotary motion about its axis of figure*, such a body will be subject to the action of two forces; *its weight*, acting at the centre of gravity, and the *resistance of the plane*, acting at the point vertically upwards.

According to the fundamental principles of dynamics, the centre of gravity will move as if the mass and forces were concentrated at that point, while the mass will turn about this centre as if it were fixed. Calling R the resistance of the plane, M the mass, and Mg the weight of the top, and z the height of the centre of gravity above the plane, we shall have for the equation of motion of the centre of gravity*

$$M\frac{d^2z}{dt^2}=R-Mg \qquad (1.)$$

As the angular motion of the body is the same as if the centre of gravity was fixed, and as R is the only force which operates to produce rotation about that centre, if we call C the moment of inertia of the top about its axis of figure, and A its moment with reference to a perpendicular axis through the centre of gravity, and γ the distance, GK (fig. 2) of the point of support from that centre; the equations of rotary motion will become identical with equations (3) (p. 53),† substituting R for Mg

$$\left.\begin{aligned} &C\,dv_z=0\\ &A\,dv_y-(C-A)\,v_z v_x\,dt=\gamma a R\,dt\\ &A\,dv_x+(C-A)\,v_y v_z\,dt=-\gamma b R\,dt \end{aligned}\right\} \qquad (2.)$$

The first of equations (2) gives us v_z as for the gyroscope, equal a constant n.

Multiplying the 2d and 3d of equations (2) by v_y and v_x respectively, and adding and making the same reduction as on p. 53, we shall get

$$A(v_y dv_y+v_x dv_x)=R\gamma d.\cos\theta.$$

* As there are no horizontal forces in action, there can be no horizontal motion of the centre of gravity except from initial impulse, which I here exclude.

† The references throughout this paper are to my paper on the gyroscope in the July number of this Journal.

But z (the height of the centre of gravity above the fixed plane) $=-\gamma\cos\theta$; hence $\gamma d.\cos\theta=-dz$; and equation (1) gives $R=M\left(\frac{d^2z}{dt^2}+g\right)$. Substituting these values of R and $\gamma d.\cos\theta$ in the preceding equation, and integrating, we have

$$A(v_y{}^2+v_x{}^2)+M\left(\frac{dz^2}{dt^2}+2gz\right)=h \qquad (3.)$$

From the 2d and 3d of equations (2) the equation (*c*) (of the gyroscope, p. 54) is deduced by an identical process.

$$A(bv_y+av_x)+Cn\cos\theta=l,$$

and a substitution in the two foregoing equations of the values of the cosines a and b, and of the angular velocities v_x and v_y, in terms of the angles φ, θ and ψ (see pp. 52, 53), and for z and $\frac{dz}{dt}$ their values, $-\gamma\cos\theta$, and $\gamma\sin\theta\frac{d\theta}{dt}$, and a determination of the constants, on the supposition of an initial inclination of the axis α, and of initial velocity of axial rotation n, will give us for the equations of motion of the *top:*

$$\left.\begin{aligned}&\sin^2\theta\frac{d\psi}{dt}=\frac{Cn}{A}(\cos\theta-\cos\alpha)\\ &A\left(\sin^2\theta\frac{d\psi^2}{dt^2}+\frac{d\theta^2}{dt^2}\right)+M\gamma^2\sin^2\theta\frac{d\theta^2}{dt^2}=2Mg\gamma(\cos\theta-\cos\alpha)\end{aligned}\right\}(4.)$$

from which the angular motions of the top can be determined. The first is identical with the first equation (4) for the gyroscope. The second differs from the second gyroscopic equation only in containing in its first member the term $M\gamma^2\sin^2\theta\frac{d\theta^2}{dt^2}$, or its equivalent $M\frac{dz^2}{dt^2}$, expressing the living force of vertical *translation* of the whole mass.

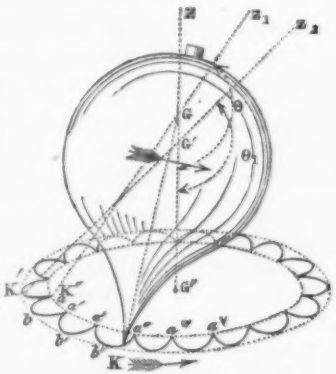
The second member (as in the corresponding equation for the gyroscope) expresses the *work of gravity*, and the first term of the first member expresses the living force due to the angular motion of the axis. Instead therefore of the work of gravity being expended (as in the gyroscope) *wholly* in producing angular motion, part of it is expended in vertical translation of the centre of gravity. The angular motion takes place not (as in the gyroscope) about the point of support (which in this case is not *fixed*), but about the centre of gravity (to which the moments of inertia A and B refer); and that centre, motionless horizontally, moves vertically up and down, coincident with the small angular undulations of the axis through a space which will be more and more minute as the rotary velocity n is greater.

An elimination of $\frac{d\psi}{dt}$ between the two equations (4) and a study of the resulting equation, would lead us to the same general results, as the similar process, p. 56, for the gyroscope.

The vertical angular motion, expressed by the variation which the angle θ undergoes, becomes exceedingly minute (the maximum and minimum values of θ approximating each other) when n is great, and the axis gyrates with slow undulatory motion about a vertical through the centre of gravity. It would be easy, likewise, to show by substituting for θ another variable, $u=\alpha-\theta$, always (in case of high values of n) extremely small, and whose higher powers may therefore be neglected, that the co-ordinates of angular motion, u and ψ, approximate more and more nearly to the relation expressed by the equation of the cycloid as n increases; though the approximation is not so rapid as in the gyroscope. All the results and conclusions flowing from the similar process for the gyroscope (see pp. 57, 58, 59, 60) would be deduced. As, however, the centre of gravity, to which these angular motions are referred, is not a *fixed point*, but is itself constantly rising and falling as θ increases or diminishes, the actual motion of the axis is of a more complicated character.

If GK'' (see fig. 2) is the initial position of the axis of the top, the motion of the centre of gravity will consist in a vertical falling and rising through the distance $GG'=GK''(\cos z_2 G'G''-\cos z_1 GG'')=\gamma(\cos\theta_1-\cos\alpha)$ (in which θ_1 is the *minimum* value of θ), while the extremity of the axis or *point*, K, describes on the supporting surface and about the projection G'' of the centre of gravity, an undulating curve a, b, a', b', a'', &c., having *cusps* a, a', &c., in the circle described about G'' with the radius $G''K''=\gamma\sin\alpha$, and tangent, externally, to the circle described with a radius $G''K'=\gamma\sin\theta_1$. But as in the case of the gyroscope, these little undulations speedily disappear through the retarding influence of friction and resistance of the air, and the point of the top describes a circle, more or less perfect, about G''.

2.



The *rationale* of the self-sustaining power of the top is identical with that of the gyroscope; the *deflecting* force due to the

angular motion of the axis plays the same part as the sustaining agent, and has the same analytical expression. Owing to *friction*, the top likewise rises, and soon attains a vertical position; but the agency by which this effect is produced is not exactly the same as for the gyroscope.

If the extremity of the top is rounded, or is not a perfect mathematical point, it will *roll*, by friction, on the supporting surface along the circular track just described. This rolling speedily imparts an angular motion to the axis greater than the horizontal gyration due to gravity, and the deflecting force becomes in excess, (as explained in the case of the gyroscope,) and the axis rises until the top assumes a vertical position. Even though the extremity of the top is a very perfect *point*, yet if it happens to be slightly *out* of the axis of figure (and rotation) the same result will, in a less degree, ensue: for the point, instead of resting *permanently* on the surface, will *strike it*, at each revolution, and in so doing, propel the extremity along. The conditions of a *perfect point*, perfectly centered in the axis of figure, are rarely combined, or rather are *practically impossible;* but it is easy to ascertain by experiment that the more nearly they are fulfilled, and the harder and more highly polished the supporting surface, the less tendency to rise is exhibited; while the great *stiffness* (or tendency to assume a vertical position) of tops with rounded points, is a fact well known and made use of in the construction of these toys.

CORRECTION.

In the No. for July, 1857, vol. xxiv, No. 70, p. 65, 8th line of note, for "vertical," *read* "horizontal," and for "horizontal," *read* "vertical."

10th line, for "the first," *read* "the second."

Art. XI.—*Review of the Operations and Results of the United States Coast Survey.*

Though the annual reports of the United States Coast Survey have been frequently noticed in this Journal, no general or connected view of the progress of our greatest national scientific work has yet been given. The voluminous reports themselves, appearing as they do each year, filled with elaborate details and copiously illustrated by diagrams and charts, convey no adequate idea of the real magnitude and importance of the Survey. We are apt in the attentive and well repaying study of the details to lose sight of the unity of the whole. So many sciences are here pressed into service, and so many special results of absorbing interest are obtained, that the materials almost make us forget the size and the beauty of the edifice.

An organized being, said Kant, is that of which all the parts are mutually ends and means. We are forcibly reminded of the definition in studying the operations and results of the Coast Survey. All the results, in geography, in physics, in geology, in short in every branch of science, are at once means and ends: means, as they form necessary and integral parts of a great and symmetrical whole; ends, as they all possess a fixed and definite value in the sciences to which they belong. This remark, if true as applied to the Survey considered simply from a scientific point of view, is far more forcibly illustrated by the practical bearings of the work, every one of whose details has an immediate practical value, while the enduring, far-reaching utility of the whole is second to that of no other human undertaking.

It is our purpose in the following pages to offer a concise view of the operations and results of the Coast Survey, regretting only that the necessary limits of a scientific review will scarcely permit us to give more than an outline sketch.

The survey of a coast so extensive as that of the United States is even in its general features a work of immense extent. That coast stretches from New Brunswick to Mexico, and from the Straits of Fuca to Old California; upon the Atlantic and Gulf from the 24th to the 44th parallel; upon the Pacific from the 42d to the 50th. Within that range of more than 5000 miles it embraces every geographical peculiarity. Innumerable islands, bays, capes and headlands, line the long reach of shore. Sand-banks hundreds of miles in length, enclosing shallow half-inland sounds, and bays stretching far into the interior like Norwegian fiords, are among its most striking features. The Coast Survey must accurately locate every prominent point, map out the bottom of every bay and harbor, fix the bearings of every reef and shoal, trace the course of every current, deduce from long continued observations the laws of the tides, and in short, observe and measure every peculiarity in the physical geography of the coast which the most refined science, the most delicate methods of observation and the most perfect instrumental means can measure or detect.

After a preliminary reconnoissance, the Survey begins with the measurement of a base, that is to say, with the accurate determination of the length of a line upon the earth's surface, the two extremities of which shall serve as starting points. Setting out from these two initial points, the survey proceeds by great steps of thirty or forty miles till the whole coast is covered with a network of large triangles, constituting what is termed the primary triangulation. The angles only of these triangles are measured, the sides being successively calculated by the aid of the angles and base. The accurate measurement of this line involves the most delicate instrumental methods. The expansions,

inclinations, and flexures of the measuring bars, and the contacts of their extremities must be observed with exquisite nicety. Here at the very first step in the work, the science of physics is called upon for its indispensable aid, and the reflecting pyrometer measures the expansions of the bars with an accuracy which is almost without limit. Delicate levels give the corrections for the inclinations and flexures, while the contact level determines the contacts of the successive bars. To such a degree of perfection have the measurements been brought that the probable error in determining the length of a base five miles in length does not under favorable circumstances exceed a few tenths of an inch. The base line being measured, the triangulation begins. Signals at distances of ten to twenty miles from either end of the base are observed in succession, and their angular distances determined with all the precision which modern mechanism has given the theodolite. The sides of the great triangles thus obtained being calculated, the signals form themselves fixed points for new angular measurements, and so the triangulation stretches from hill to hill till the prominent points of the entire coast are determined. The signals themselves are of no small interest. Bright tin cones mounted upon poles are often used, and reflect to a distance a brilliant line of solar light. But of all signals, the heliotrope—a movable mirror placed so as to be directed by a telescope—is the most perfect. With this instrument, the sun's rays have been reflected so as to be distinctly seen with a telescope at the distance of more than one hundred thousand yards. It is easy to see that, as the triangulation extends, the small errors inseparable from every physical measurement may accumulate so that the positions of the stations most remote from the base line may be incorrectly determined. Two methods are adopted to guard against errors of this kind. A subsidiary base line may be measured near the terminal signals of the primary triangulation, and from the extremities of this we may work backwards, so as to check the results of the first series of observations and calculations of distances. The geographical positions of the principal stations may themselves be determined by accurate astronomical observations, assigning their exact latitudes and longitudes, to check the calculated positions. Both these methods have been brought almost to perfection, but the latter has been especially fertile in new and beautiful results.

It is easy to see that while the bases and triangles give distances, the observations of latitude and the azimuths give differences of longitude by the aid of spheroidal formulas, a central longitude having been once determined. The differences of longitude are in their turn checked by the telegraph operations to which we shall presently allude.

The base which serves as the commencement of the primary triangulation for the eastern and middle states lies within the state of Massachusetts, a little to the north of Rhode Island. Its length is over ten miles and its direction nearly northeast and southwest: it was measured in 1845. Surveys for a verification base have been made in the northeastern part of the state of Maine, on Epping Plains. Since the commencement of the survey not less than nine primary and thirty-five secondary bases have been measured, making a total length of about 135 miles.

As the different stations of the primary triangulation are at different heights, it is necessary to measure vertical as well as horizontal angles, and finally, in consequence of the spheroidal figure of the earth, to reduce the plane triangles, which are the direct results of the measurements, to spheroidal triangles, at the level of the sea. In this manner after immense labor, both of observation and of calculation, the positions of the primary stations are at length fixed, and these now serve as starting points for the secondary triangulation which determines the general outline of the coast in detail, and the positions of rocks, reefs, and islands. The triangles observed are now smaller but very much more numerous, and the labor of observation and reduction even greater than before. Then comes the topography of the coast, the work at every successive step running more and more into detail. The coast line is now traced and laid down in charts of elaborate minuteness and finish. Harbors are surveyed and mapped out by innumerable soundings; the exact character and value of each being determined. The nature of the bottom with reference to anchorage, the depth and direction of channels, currents, tides and prevalent winds, the proper position of lighthouses, buoys and fog-bells, all form subjects of special and minute attention. In this manner the entire coast from Mt. Desert island to Cape Fear has been almost completely surveyed, and that portion of the sea-coast may be regarded as nearly finished. But beside the bays, harbors, and sounds of the coast, the rivers receive their share of attention, small triangulations being carried up to the head of tide waters, based upon one of the sides of the larger work. In this manner the river shores are accurately mapped, while careful soundings determine the bars and channels. The mouths of the larger rivers offer special subjects of examination of the highest interest and importance. We refer to the changes in the depth and position of the channels produced by the effects of currents. The characteristics of the delta of the Mississippi, and the enormous quantities of matter annually brought down by the current are too familiar to require notice in this place, but the changes in the entrance to the harbor of New York have not until very recently attracted attention to the same degree.

The extension of docks and piers into the North and East rivers, and the amount of new made land, excited such serious alarm in the mercantile community that a Board of Harbor Commissioners was appointed in the year 1855 to consider the whole subject and devise means for averting the impending danger. To the action of that Board the assistance of the Coast Survey was cheerfully and gratuitously rendered, and the results of the survey and of a careful and accurate examination of the effects of wharves and piers upon harbor currents, have been of the greatest importance to the commercial prosperity of the city. Another very important observation has been made of the existence of a slowly moving northwardly current on both sides of Sandy Hook, tending by deposits of sand to narrow the main ship channel. For the action of this current the Survey provides a remedy which can be applied as soon as it shall become necessary.

We have already stated that the assistance of astronomical observations is needed in the operations of the primary triangulation. The special objects of the Survey and the peculiar character of the work done by it, have in their turn exercised a remarkable influence upon astronomy itself, considered at least as a science of observation. New methods for the determination of latitudes and longitude have had their origin in the necessities of the Coast Survey alone, while the methods already known and practised have been developed and perfected. Special series of observations have been undertaken at the two observatories of Cambridge, Mass., and at numerous minor stations, to obtain the astronomical data requisite for the determination of differences of longitude, while the genius of the most eminent mathematician whom our country has produced, has been devoted to the perfecting of the methods of reduction and calculation.

The determination of differences of longitude by means of the electric telegraph—a method which doubtless suggested itself to thousands, when the telegraph passed from an idea to a reality—was first carried out properly by the Coast Survey. This method has not merely yielded results of great value to the survey itself, but the particular mode of applying it has been generalized into the American method of recording astronomical observations, and of measuring minute intervals of time. This method has been adopted in several European observatories:—in all but one with an honorable acknowledgment of its origin.

The experiments of Mr. Wheatstone made in 1834 with apparatus devised and executed by Mr. Saxton, gave the first determination of the velocity of electricity in a metallic conductor. This velocity was estimated at about 288,000 miles per second, and until within a recent period, in spite of all analogy

and even of experience, Wheatstone's determination was supposed to hold good, at least approximately, for galvanic currents as well as for electricity of high tension, and for bad as well as good conductors. The telegraph operations of the Coast Survey demonstrated at the very outset the inaccuracy of received ideas upon this subject: they shewed that the galvanic current moves very much less rapidly than electricity of tension, varying according to Dr. Gould's discussion of the observations, from 12,000 to 18,000 miles per second, and though not yet fully reduced, they have rendered it, to say the least, probable that the velocity also diminishes as the resistance to conduction increases. Special investigations directed to all these points have been undertaken, and a volume of records and results is now preparing.

To determine the difference of longitude between Cambridge, Mass., and Liverpool, four distinct chronometric expeditions have been sent out, namely: in 1849, 1850, 1851, and 1855. In the last expedition the number of voyages made was six, and the number of chronometers sent out fifty-two. The first three chronometric expeditions gave results which could not be considered as satisfactory, in consequence of the remarkable influence exerted upon the chronometers by differences of temperature, though the instruments were compensated in the usual manner. In the expedition of 1855 an uncompensated chronometer was employed with the others. Great care was taken to observe the temperatures of the chronometers themselves during the voyage by means of thermometers, and the results of the compensated instruments were carefully compared with those of the single one without compensation. The chronometers employed were moreover carefully investigated before the expedition, by exposing them to different degrees of heat and noting the effect upon the rate. The observations of this last expedition, after careful discussion, have given a difference of longitude between Cambridge and Greenwich of $4^h\ 44^m\ 31^s{\cdot}89$, with a probable error of about 0·2 of a second. The successful laying of the telegraph wire between Europe and America is anxiously looked forward to as presenting an opportunity for determinations of longitude by the most perfect method which human ingenuity has yet devised. The importance of this single determination can scarcely be over-estimated, since it fixes the geographical relations of the old and new worlds.

In connection with its astronomical and geodetical observations, the Coast Survey has been enabled at a trifling expense to carry out an extensive series of determinations of the three magnetic elements at very numerous stations. These elements it will be remembered are the declination, inclination, and horizontal intensity. So great has been the amount of material ac-

cumulated, that the report for 1856, now in process of publication, contains for the first time, magnetic charts of the North American continent sufficiently complete to enable us to compare upon the large scale for our own country, the results of observation and of theory. In these charts the lines of equal variation, dip and intensity are traced, and a comparison of the two former with those deduced from the general theory of Gauss exhibits a satisfactory agreement in form. Further observations, especially in the interior, are wanting to enable us to make this comparison quantitatively, as well as qualitatively. The determinations of the declination and variation at points actually upon the coast, are of great service in navigation. The report of 1856 contains also very valuable and elaborate discussions of the secular variations of magnetic declination and inclination, both upon the eastern and western coasts, reference being had to the earliest recorded observations. Empirical formulas are also given for a number of stations, which enable us to determine with a satisfactory approximation, the two angular magnetic elements at any required epoch.

In the progress of the hydrography of the Coast Survey, very numerous observations of the tides have been made for the purpose of correcting soundings, and of determining the establishments of the different ports. The necessity of both these classes of observations is sufficiently obvious, but the superintendent has connected with them observations of a more permanent character, in order to ascertain the laws of the tides in particular localities, and to trace the progress of the tide wave along the coast severally, as well as in bays and rivers. For these observations two species of gauge have been used; the self-registering and the common staff gauge. The former possesses the advantage of requiring little or no attention, and of furnishing a permanent and reliable record in the form of a curve of which the abcissas represent the times and the ordinates the corresponding heights of water. The number of principal stations for tidal observations was 73, of which number 45 were on the Atlantic Coast, 18 on the Gulf, and 10 on the Pacific.

Since the commencement of the survey there have been in all 613 tidal stations, of which however a comparatively small number have been furnished with self-registering gauges. The results obtained have been of great practical importance, giving as they do for the first time accurate information with respect to the tides of the Gulf and of the Pacific coasts, and leading to the construction of accurate and reliable tide tables.

It is not saying too much to assert that no single series of tidal observations yet made possesses so high a scientific value as those of the Coast Survey. Not only is the range of coast studied greater, but the character of the tides themselves is in a

great measure sufficiently free from the effects of local causes to enable us to obtain from them results of definite value for the general theory. On the other hand the Gulf of Mexico and particular portions of the Atlantic Coast exhibit peculiarities of much interest, as yet imperfectly investigated, but seeming to show the importance of a careful study. While the tidal observations hitherto discussed have been for the most part isolated, made at different points upon the earth's surface by individuals, during a period of about 200 years, those of the Coast Survey have been made systematically, at numerous carefully selected stations, upon the coasts of a continent lying between two great oceans, and under the direction of a single person.

An elaborate discussion of these observations has led to the construction of maps of the cotidal lines of the Atlantic, Gulf and Pacific coasts, which are of especial interest not merely from their connection with our own shores, but from the fact that they are the only series of cotidal lines yet deduced from an extensive and connected series of observations. The term "cotidal line" it will be remembered was first introduced by Mr. Whewell to denote a line passing through all those points which have high water at the same hour of the day. It is convenient to assume twenty-four such lines, and they may obviously be regarded as forming the crests of successive advancing tide waves. Their shape, velocity, and direction of motion, will depend upon the configuration of the coast, the depth of the ocean and the various local causes which disturb the uniformity of their progress and cause divisions and interferences of divided waves. Were no disturbing causes present, the cotidal lines would correspond with the meridians, each line at a certain distance behind the meridian of the moon at its culmination. It is easy to see too that the cotidal lines must differ upon the eastern and western shores of a continent like that of North America, since the tide wave moves from east to west and is therefore upon the eastern coast an incident and upon the western a receding wave, the character of which is determined by the flow of water and its pressure from north, south and west. The cotidal lines of the Atlantic coast follow the general outline of the coast itself in a remarkable manner, the velocities measured in a direction perpendicular to the front of the waves varying from 24 to 40 miles per half hour. The tides on the Atlantic coast are of the regular semi-diurnal class; the diurnal inequality is not large and generally difficult to trace, though easily recognized at particular periods. On the Gulf coast, on the contrary, the tides are small, the semi-diurnal being masked by the diurnal waves. The tides of the Pacific coast are remarkably regular, both in the diurnal and semi-diurnal waves, and moreover rise to such heights as to render observation easy. Throughout the extent of coast exam-

ined, the cotidal lines for the Pacific are either sensibly parallel to or make but a small angle with the coast.

Tide tables for the principal sea ports of the United States have been published by the Superintendent of the Coast Survey by authority of the treasury department; they are based exclusively upon the observations of the survey, and will be extended and corrected as the survey advances. Meantime their value to navigators places them among the important results of the Coast Survey.

The tidal observations of the Pacific coast have casually led to a determination of great scientific interest, that of the average depth of the Pacific Ocean between the coasts of Japan and California. On the 23d of December 1854, an earthquake occurred in Japan by which the town of Simoda in the island of Niphon was destroyed. From the imperfect accounts which have reached us it appears that at 9 A. M. on that day the severe shock of an earthquake was felt on board the Russian frigate Diana, then lying in the harbor of Simoda. Half an hour later the sea came into the bay in an immense wave thirty feet in height, overwhelming the town and then receding. This advance and recession occurred five times, and by 2·30 P. M. all was again quiet. The depth of the sea during these changes varied from less than eight to more than forty feet. Upon the same day an extraordinary rise and fall of water was observed at Peel's Island, one of the Bonin Islands, and the tide continued to rise and fall during the day at intervals of 15 minutes, gradually lessening until evening.

The self-registering gauges at San Diego and San Francisco, exhibited on the 23d and 25th of December, remarkable irregularities of the tidal curves. Each gauge exhibits three sets of waves at short intervals, and there can be no doubt that these waves were produced by the same cause which determined the rise and fall of the ocean at Japan and the Bonin Islands. No record of the occurrence of an earthquake in Japan on the 25th of December has yet reached us, but the waters rose on the evening of that day at Peel's Island to the height of 12 feet. The distance of Simoda from San Francisco is 4,527 nautical miles, and from the same port to San Diego 4,917 miles. From these data and the times at which the disturbances occurred upon the two coasts, the waves from Simoda to California would move at about the rate of 360 miles per hour or 6 miles per minute, and would have a length of about 200 miles. This would give for the average depth of the Pacific about 2,200 fathoms or rather more than two miles. The publication of the official report of Admiral Pontiatine who commanded the Diana will probably permit more accurate determinations and explain the origin of the disturbance upon the 25th of December.

(*To be concluded.*)

ART. XII.—*The Open North Polar Sea;* by R. W. HASKINS, A.M.

THE physical condition of our globe, though intimately connected with the daily walk and welfare of man, is a subject which never has occupied more than a very slight share of the popular attention. There are features, however, of this condition, which occasionally force themselves upon the attention of all men; though seldom for more than a brief period, and then only as an element of alarm or of idle curiosity, rather than as one of investigation, and as forming the basis of knowledge. Such are earthquakes, sudden eruptions of volcanoes, and the like. The more fixed and stable forms of surrounding nature, as they lack the stimulant of unusual and violent change, can excite, in the public mind, none other than the most feeble attention, and that only under circumstances of specific incentives. Among the direct consequences of this state of things is a constant propensity to generalize, and to base ultimate conclusions upon appearances only, and with none other than a superficial observation of these. It is to such a propensity, and to the apathy it so naturally produces, that we may, perhaps, most safely ascribe the present condition of the popular mind, in regard to the immediate object of this notice.

That to recede from the equator towards the poles, upon the surface of the earth, is to encounter increased cold, is a *general* fact, well known to all; and it was easy for even the most drowsy quietude to *infer*, from this, that the law thus known is a constant one admitting of no exception, and that, consequently, the geographical pole must be the coldest point of our globe. How far the votaries of science may have concurred in this sentiment, and even contributed to its establishment, it is, of course, quite impossible to determine. It is however a fact, that all the later meteorological charts place the regions of greatest cold on the continents, and *not* within the polar sea.

This being the state of the public mind—a state wholly undisturbed by any efficient teachings—the announcement, by the late Dr. Kane, on his return from his northern explorings, in 1855, that there existed, and that some of his people had seen, an open sea or ocean in north latitude 82° 30′, was very generally received with astonishment; and it very widely produced that species of undefined amazement, which might well belong to assumed phenomena in nature, that had hitherto been deemed physically impossible. However little creditable the fact just stated may be deemed to the mass of those whose standard of intelligence, upon the physical condition of our globe near the North Pole, is measured and fixed by it, yet most certainly those who had made the physical condition of

the earth's surface a reasonably successful study, had ample cause for real astonishment at the great extent to which all the teachings of the past, in regard to this open northern polar sea, had been either overlooked or forgotten.

Since the occasion in question has fixed so much attention upon this open sea as a new thing to us all, and is still employing so many pens upon it as such, it seems a fitting time to place the more general public in possession of the past knowledge of this sea, in a collected form, by way of giving profitable direction to the laudable public zeal which is just now so earnestly manifested in the case.

This unfrozen polar sea, then, has been long known and often navigated, at different periods, and by different nations and vessels. The earliest no less than the most persevering navigators of high northern latitudes, were the Hollanders or Dutch and the Greenlanders. These people did not resort to these latitudes year after year for the purpose of scientific discoveries of any kind. Their purpose was whale and seal catching, and to whatever regions they penetrated, they were led solely by the pursuit of these creatures. Now it is from these early navigators thus employed, and chiefly from the log-books of their ships, that we have derived almost all we know of a constantly open sea about the north pole of the earth. Nor is the peculiarly reliable nature of this testimony to be overlooked. It comes, as we have said, from men who had no theory to sustain or combat, and no end to serve or aim to accomplish, by falsifying or perverting the record. Again, this record of the log is not one of an afterthought that may be made to conform to events that have transpired after its date, but it is one that is written out daily and at the period of its date—thus constituting a daily and even hourly record of events as they transpire on board. Of the great mass of this species of evidence that has doubtless been brought home by the ships from the polar regions, we may well suppose we possess but a very small proportion, since all we have owes its preservation either to accident or the individual efforts of devoted men. In proceeding to cite the evidences we possess in this matter, we may premise that a large portion of them were collected from their various sources by the Hon. D. Barrington, and by him published at London in 1776. As the citations we are about to make are numerous, and also cover a very considerable range of time, we have thought to add to their more clear understanding by arranging them in their natural or chronological order, so far as it has been possible to ascertain this.

Davis, an English navigator, who was sent north with two ships in 1585, to discover a northwest passage, and who did discover the straits that bear his name, is, by modern authors,

credited with having reached only 66° 40′ north latitude, while Camden, in his annals of Elizabeth, asserts that Davis attained to 83°.

Moxon's account of a Dutch ship that sailed to the pole, and even beyond, is this: Being, says he, about 22 years ago, in Amsterdam, I went into a drinking house to drink a cup of beer for my thirst, and sitting by a public fire among several people, there happened a seaman to come in, who seeing a friend of his there who he knew went in the Greenland voyage, wondered to see him, for it was not yet time for the Greenland fleet to come home, and asked what accident had brought him home so soon? His friend (who was the steerman [mate] aforesaid in a Greenland ship that summer) told him that their ship went not out to fish that summer, but only to take in the lading of the whole fleet, to bring it to an early market. But, said he, before the fleet had caught fish enough to lade us, we, by order of the Greenland company, sailed unto the north pole, and came back again. Whereupon (his relation being novel to me) I entered into discourse with him, and seemed to question the truth of what he said, but he did insure me that it was true, and that the ship was then in Amsterdam, and many of the seamen belonging to her, to justify the truth of it; and told me, moreover, that they had sailed two degrees beyond the pole. I asked him if they found no land nor islands about the pole? He told me no, there was a free and open sea. I asked him if they did not meet with a great deal of ice? He told me no, they saw no ice. I asked him what weather they had there? and he told me fine, warm weather.

This conversation, &c. at Amsterdam was about the year 1624, at which time the vessel had lately returned. Moxon, who related this statement, was not an obscure nor an illiterate individual, since in the title to his published statement he calls himself Fellow of the Royal Society, and Barrington states that he was Hydrographer to Charles the Second, and author of several scientific papers. It was probably his professional calling, therefore, that fixed his attention upon this subject, and thus caused his inquiries at Amsterdam. A map was early published by the Academy of Sciences at Berlin, which places a ship at the pole, as having arrived there, according to the Dutch accounts. We are not aware that the date of this map is preserved, but it seems probable that one of the authorities for that ship's position is the account of Moxon, cited above.

Wood sailed on the discovery of a northeast passage to Japan, in 1676; and in his account of his voyage, which he subsequently sent to the press, he says he was chiefly induced to the undertaking by the account given by Capt. Goulden, of a Dutch ship, who had made some thirty voyages to Greenland. This

Captain's statement was, that being in company with two other Holland ships to the eastward of Edge's Island, in pursuit of whales, and these not appearing there, the two Hollanders resolved to go farther north. They did so, and at the end of two weeks returned again and said they had sailed to latitude 89°; and when Capt. Goulden doubted this, they produced out of the two ships four journals or log-books, which confirmed the statement, and did not differ from each other in all but four minutes of a degree. In this run to the north they encountered no ice, but had a free and open sea. This occurrence is stated by Capt. Goulden to have taken place some twenty years before its narration to Wood, which places it somewhere about the year 1650.

In 1662 Mr. Oldenburgh, Secretary of the Royal Society, London, was ordered to register a paper entitled "Inquiries concerning Greenland, answered by Mr. Grey, who had visited those parts." To the question "How near hath any one been known to approach the pole?" Mr. Grey answered,

"I once met [date not given, but of course prior to 1662] upon the coast of Greenland, a Hollander that swore he had been but half a degree from the pole, showing me his log-book, which was also tested by his mate; where they had seen no ice or land, but all water."

Dr. Campbell, who compiled Harris's Travels, states therein this: "By the Dutch journals they get into north latitude 88° 56′, and the sea open." On being asked his authority for this statement, Dr. Campbell answered that he received it from Holland as being an extract from the journals produced to the States General, in 1665.

Martin, in his voyages, says, "in 1671 we sailed to the eighty-first degree, and no ships ventured farther *that year*."

Dr. Dallie, a native of Holland, who resided in Racquet Court, Fleet Street, as a practising physician, about the year 1745, stated to Dr. Campbell, who is mentioned above, that when he was young he sailed in a Dutch man-of-war that was sent north to protect the whale fishery, and that the ship, on that occasion, reached latitude 88° north, where the weather was warm and the sea wholly free from ice. The date of this voyage may have been about 1685.

Jacob Schol, who resided at the Helder, in 1700 sailed to 84° north in pursuit of whales, and had open sea there without obstruction.

Andrew Fisher, an English captain, who had made twenty-four voyages to the Greenland seas, testifies that, in the year 1746, in the ship Ann Elizabeth, from London, he sailed to 82° 34′ north, where he met a loose pack of ice. He fished there, and went no higher north, but had no doubt he might have done so, through all the ice there was, had he been so minded.

In 1751 Capt. MacCallam, in the ship Campbeltown, in the Greenland whale fishery, sailed to latitude 83° 30′ north, where the sea was not only wholly open at the north, but where he had not seen a particle of ice *for the last three degrees*, and the weather warm and pleasant. In this case, to make certain of their position, careful observations were made, both with Davis' and with Hadley's quadrants, and by no less than three different persons. The captain feared to go farther, lest he should be blamed for neglecting his fishing, which was his only reason, as there was no obstruction.

In the year 1752, Mr. John Phillips was mate of the ship Loyal Club, in which ship he reached 81°; and he stated that it was very common to seek whales in such latitudes.

The year 1754 was more fruitful than any prior one in recorded visits to this open polar sea, since we have records of no less than three such visits during this single year. Capt. James Wilson, of the whale ship Sea Nymph, made his way through all the ice, the last of which was seen below 81°, sailed thence north to 82° 15′, where the sea was perfectly clear as far as could be seen with the ship's glasses. Here the ship's officers discussed the proceeding directly to the pole, but the sailors fearing to do so, the proposition was abandoned. In the same year the whaling ship Unicorn, Capt. Guy, reached 83° 3′, determined by careful observations; and here, from the mast head, they saw the sea as free of ice as the Atlantic, on every side, and nothing in the way of sailing directly to the pole. The third instance of this year is that of Mr. Stephens, who, in company with another, a Dutch ship, was driven off Spitzbergen by a south-southeast wind to latitude 84° 30′. This was within 5° 30′ of the pole; and he met with little ice, and the less the farther he went north.

In 1756 Capt. Montgomery, of the ship Providence, pursued whales to latitude 83°, in the month of June, with open sea upon the north.

In 1759 Capt. H. Ford, in the ship Dolphin, went as far north as 81° 30′, and he states that he has since that been several times as high as 81°.

James Bisbrown, in the ship Prince Frederick, in 1765, reached latitude 83° 40′ north, where he was beset with ice for three weeks, *to the southward*, but saw, during this time, open sea to the north.

The year 1766 has furnished us two instances of high northern navigation. Jonathan Wheatley, not finding whales sooner, sailed to 81° 30′ north, in which latitude he could see no ice whatsoever in any direction from the mast head, though there was a very heavy sea from the northeast. Capt. Thomas Robinson, in the ship Reading, was this same year in latitude 82° 30′, with an open sea.

In 1767 Samuel Standidge sailed from Hull, England, on board the ship British Queen, of which he was owner but not master, for the north seas. On the sixth of May this ship reached latitude 80°, "which," says the narrator, "is near what the masters call a fishing latitude;" and he adds, "I found the farther north the less quantity of ice."

For the year 1770 we have two instances to cite. James Marshall, as mate on the ship Royal Exchange of Newcastle, was this year in latitude 82° 30′ north. Capt. Brown, of the ship Freelove, states that he was in latitude 82°, this same year, and that the sea was then all clear.

The year 1771 seems to have been a favorable one for high northern penetration by water. Capt. Jan Klass Castricum, in the ship Jonge Jan, fished with success in latitude 81° 40′, in company with a ship from Hamburgh. There were, also, two English ships fishing there; and these went still farther north, until they were out of sight from the mast head. They were gone thus between two and three days, and when they returned their captains came on board Castricum's ship, and assured him that they had been as far as 83°.

In 1773 Capt. John Clarke, of the Sea Horse, went to 81° 30′ where he had an open sea to the north, and a heavy swell from the northeast, with a fresh wind. Capt. Bateson, of the ship Whale, on June 14th of this year, was in north latitude 82° 15′, in pursuit of whales, and with no complaint of ice.

The *Journal des Sçavans* for the month of October, 1774, states that an officer in the English service had then in his keeping the journals of a Greenland ship wherein it stands recorded that said ship, in the month of May—the year not given, but supposed to be then recent—had been in latitude 82° 20′ north, and that the sea was open.

This mass of testimony, it is seen, has been chiefly gathered from foreign languages, and been furnished by other than English navigators. The reason of this is that the English were not the early navigators of these high latitudes. Those hardy adventurers were Hollanders and Greenlanders, who, jointly, long monopolized the whale fishery in the Arctic seas. No general motive or ambition, then, has existed in England to preserve and promulge the details of the voyages made by these people; and hence we know not what proportion of such material as the above may be still unknown to us by having been lost. Certain it is that many instances of such are found, scattered here and there, which are not so fully authenticated as to seem deserving of a place here, and they have therefore been excluded.

Since the concluding date of the foregoing list of proofs of an open sea at the north pole, we have no evidence that ships have penetrated that sea as they formerly did. Modern explorers,

which, within the last forty years have been numerous, have not done so, having invariably been stopped by ice, and usually at much lower latitudes than where this open sea has ever been known to extend. But, although these modern explorers have not reached that sea, and sailed their ships upon it as their predecessors did, still some of them have brought us as demonstrative proof of the existence of that open sea as if they had actually floated thereon. One of these is Capt. Parry, who wintered at Melville Island, in latitude 74° 45′ north. He tells us that there a *north* wind, in the long winter of that frozen region, modified the cold, and if continued, produced a thaw. Now this single fact, if well established—and we take this one to be—without one particle more of evidence, would establish beyond all doubt or controversy, the existence of an open sea, in the direction whence that wind came. Such an effect from a wind is wholly incompatible with the assumption that it has passed only over a *frozen* surface. This statement of Capt. Parry is fully confirmed by other proofs—or rather those other proofs, being of prior date, are confirmed by it. Barentz, when his ship was frozen in Nova Zembla, heard the ice broken, with a most frightful noise, by an impetuous sea, from the north; while the Samoides and Tartars, who live beyond the Waygat, all believe, and we must suppose from like reasons, that the sea is open, on the north of Nova Zembla, all the year. The testimony of persons who have passed the winter at Kola, in Lapland, coincides perfectly with this, namely, that, in the most severe weather, whenever a northerly wind blows, the cold promptly diminishes, and that, if the wind continues, it always brings on a thaw, as long as it lasts.

If we ask why these more recent navigators could not reach the high latitudes their predecessors did, the only and the sufficient answer is, that the icy barriers which always exist *on the way* to this open sea and south of it, vary greatly in solidity and width in different years. These barriers have always been passed through by those who have entered the open polar sea, and they have often proved too broad and solid to be penetrated at all. The fact of these wide differences in the extent and strength of the ice in the northern seas in different years, is attested by all we know of the regions in question, whether by land or sea. All the history of Greenland attests it, and the fact is no less constantly proved by the experience of northern whalers. To cite a single case in illustration, and that a recent one, we may mention that of Capt. Parry. This navigator, during his third voyage in 1824, found the icy barrier in Baffin's Bay *one hundred and fifty miles* broader than when he passed it in 1819. The differences, then, that exist in these icy barriers, in latitudes *south* of the open polar ocean, and in the most favorable seasons

of different years, sufficiently account for both the successes and the failures of navigators in reaching that open sea; while the heavy ocean swell, and the warm winter wind, both of which approach this icy barrier upon the *north*, and that, too, during the fiercest frosts of the northern sunless winters, appear to prove that the ocean towards the north pole is, even then, still open, and that it warms and tempers those winds which pass over it, and which so constantly drive its waves against that icy rampart by which the frost king has fixed and defined its southern shore.

ART. XIII.—*Correspondence of M. Jerome Nicklès, dated Paris, August*, 1857.

Obituary.—Cauchy.—In my last communication I gave some biographical details respecting the great mathematician Cauchy. The recent publication of a notice by Biot, one of his cotemporaries and friends, leads me to return to the subject.

Cauchy was born on the 21st of August, 1789. At an early age he was distinguished by great versatility of talents. His classical education, commenced by his father, was continued under able professors at the "Ecole Centrale" of the Pantheon. He left the school at the age of fifteen, after two years of literary studies, taking the second prize in Latin composition, the first in Greek, and the first in Latin verse. On account of this so universal success, the Institute decreed to him the highest honor reserved for the student of the central schools most distinguished in classical literature.

After two years at the Polytechnic School, he left it to become an engineer in the Department of Roads and Bridges. On the 6th of May, 1811, at the age of twenty-two, he presented to the Institute a memoir of remarkable character, on geometric polyhedrons, in which he generalized a theorem of Euler and completed the theory of a new species of regular polyhedrons discovered by Poinsot. M. Legendre, the most severe of our geometricians, regarded the memoir "as the production of an adept whose ability promised the highest success." He engaged the young author to pursue this line of research, and to endeavor to establish a theorem equally applicable to certain polyhedrons embraced in the definitions of Euclid for which no demonstration had yet been made out. Cauchy accomplished this in 1812. In his report thereon to the Academy, Legendre expressed his approbation with an earnestness quite unusual for him.

These two earliest memoirs seemed to show a special aptitude for problems purely geometrical. But it was soon evident that his capacity was far wider. In the years 1813 and 1814, Cauchy

produced two remarkable memoirs in transcendental analysis; in 1815, he published his memoir on the theory of numbers, in the course of which he demonstrated in full a theorem announced by Fermat, a theorem which had hitherto been demonstrated only in some of its particulars by mathematicians most skilled in these departments, as Gauss and Legendre. The Academy proposed this year, as a subject for the great mathematical prize,—To establish the theory of the propagation of waves on the surface of a heavy fluid, and of indefinite depth. Cauchy resolved the question completely. His memoir was crowned in 1816, and bore this epigram from Virgil, "Nosse quot Ionii veniant ad littora fluctus," (Georgics II,) a very happy selection, as the line contains a complete and altogether exact announcement of the proposed problem.

Successes so rapid and fertile for a young man of 27 years, assured him the first place that should become vacant in the Mathematical Section of the Institute. A circumstance which is an occasion of regret to science and himself, introduced him officially into that body. On the return of the Bourbons, a royal ordinance of the 21st of March, 1816, reëstablished the Academies under their primitive designations, of Académie Française des Sciences, des Inscriptions et Belles-Lettres, des Beaux-Arts; and carried out the new organization. In the Academy of Science, two celebrated names, those of Carnot and Monge, were replaced by two new names, those of Breguet and Cauchy.

Towards the end of 1813, Cauchy was named Adjunct Professor of Analysis at the Polytechnic School. He became full Professor in 1816. He was eminently a man of duty. Called to instruct, he turned all his thoughts to instruction. From 1816 to 1826, he published his course of Algebraic analysis, Differential Calculus, and Application of Infinitesimal Analysis to the theory of Curves,—three excellent works, well arranged, proceeding with demonstrations that are both rigorous and rich in new details. In this period he also published a memoir on the Integrals taken between imaginary limits—a subject that has given rise to several important works among our young geometers.

In 1826, he undertook the publication and authorship of a periodical review, styled "Exercises Mathematiques," in which all departments of mathematics, elementary as well as transcendental, were treated with so much generality, fertility and inventive power, that Abel, one of the profoundest analysts of our times, after reading one of these publications, wrote to a friend, "Cauchy is the geometer who best understands how mathematics should be studied." In fact the inventions of new methods and devices scattered through these "Exercises," have been not only for the author, but also for many other geometers,

the fertile initiatives of numerous brilliant works. Cauchy continued the publication of this Review until his death.

The revolution of 1830 interrupted his quiet life. At this time he was married and the father of two daughters. Besides his professorship in the Polytechnic School, he had a chair in the Faculty of Sciences at Paris, and was supplying the course of mathematical physics in the College of France. The new government imposed an oath of allegiance on all its officers, even those engaged in teaching physics and mathematics. Cauchy quitted his place and went to Switzerland. The king of Sardinia, informed of his voluntary exile, created for him in the University of Turin, a special chair of mathematics, which he filled with distinction, still continuing his other labors. In 1832 he left this chair, having been called to Prague by the ex-king, Charles the Tenth, for the education of the Count de Chambord. His wife and daughters joined him there, and with him followed the Princes to Goritz; during six years at this place he prepared a large number of valuable memoirs, which are now spread over Germany. Towards the end of 1838, his duties as preceptor terminated, and he separated from his scholar and returned to France, where he took his place among the members of the Institute. From this time, without distractions from professional duties, and diverted only by labors of beneficence or of a moral nature, he gave full freedom to the activities of his mathematical genius. During the last nineteen years of his life, he published in the volumes of the Academy and the Comptes Rendus over 500 memoirs, besides numerous Reports on memoirs presented by others. In this mass of work, so rapidly produced, many portions have their great value wrought out complete, while others present the initiatives of ideas which have either already proved fertile in results, or will yet do so. They treat of the highest subjects in mathematics:—the perfecting and extension of pure analysis,—the direct determination of the planetary motions and their most complex inequalities,—the undulatory theory of light, considered in its entire generality. Unhappily, his precipitation in producing did not leave him time to bring his results to full maturity. Each new path opened to him excited the deepest passion, and into it he plunged, neglecting what he had been exploring, even without taking the time to recognize where his methods were conducting him. To work with greater rapidity, he employed a notation full of unusual abbreviations, which render his manuscript calculations unintelligible to any one but himself. The exuberance of his genius needed, to bring out its greatest results, to be engaged in some special line of duty; and a chance was soon offered him.

In 1840, the death of Poisson left vacant a place in the Bureau of Longitudes; and Cauchy was nominated unanimously by the

board. But it was evident to all that Cauchy would not and could not take the oath, and so his nomination was not ratified by the government. It was to the loss of science; for with astronomical labors thus made his duty, he would have carried into them his usual ardor, and the "Mécanique Céleste" would probably have been advanced by new discoveries, for which we shall now have long to wait.

It was his fidelity to a sense of duty, which was afterwards the occasion and cause of his rendering a great service to astronomy, in furnishing it with the means of estimating, directly, by analytical formulas universal and certain in their application, the secular inequalities of planetary movements, which inequalities render any tables of these movements more and more faulty. In 1843, Cauchy was charged by the Academy with verifying the determination of an inequality of this kind which M. Le-Verrier announced he had discovered in the motion of the planet Pallas, the period of which embraced 795 years. It was highly important to know it, for its maximum effect on the longitude of the planet exceeded fifteen minutes of a degree, according to the valuation of LeVerrier. A direct analytic process being impracticable he secured the desired result by a very bold numerical interpolation, which required immense calculations. To relieve himself from the labor of verifying such an array of numbers, Cauchy invented an analytic method by which errors of this sort are determined directly, in all cases and with a precision in proportion as they belong to a higher order. He thus reproduced the results of LeVerrier; and henceforth in problems of this nature, the power of abstract science will supersede individual labor.

After the revolution of 1848, the Republic, more tolerant than the preceding Monarchy had been, restored to Cauchy the mathematical chair in the Faculty of Sciences of Paris, the only one of the ancient professorships which had remained vacant from 1830. Justice to him requires that it should be told that he gave to the poor the emoluments of the place.

M. Biot concludes his sketch with the following remark: "The view which I have given of the external circumstances of the life of Cauchy, shows us not only what he was, but also what he might have been as a mathematician. Had he been able, like Euler and Lagrange to spend his life, without disturbance, in quiet study, he would have been one of the grandest lights of mathematical science. By reason of the irregularity and disorder which external events impressed on his genius, his influence on this science will not be fully appreciated until time shall have developed all their consequences."

Note.—The preceding is derived from the article by Biot. We add to it the following.

In 1851, after the fall of the Republic, Cauchy was again under the necessity of suspending his duties at the Faculty of Science, since he would not take the oath of allegiance. Happily, this suspension was of short duration. On the proposition of the minister of Public Instruction, M. Fortoul, the oath in his case was dispensed with, and consequently he resumed his office and held it till his death. What is not stated by Biot, we may say—that it was at Biot's demand that this act of justice was brought about. M. Fortoul, who was then candidate for the Academy of Inscriptions and Belles Lettres, of which Biot was one of the most influential members, having called on Biot with reference to his appointment, the latter profited by the occasion to inform the Minister how happy it would be for science, if scientific instruction were not sacrificed to political considerations, so far at least that the great men of science should not be required to take an oath against their consciences; and among these last, Biot mentioned Cauchy. Three days afterward, Cauchy was excused from the oath, and thus justice was done.

Anesthesis.—Amylene.—The use of amylene in anesthesis has met with a serious check in the death from the use of it of a young man of 24 years, while in robust health. Dr. Snow put him to sleep in order to remove an epithelial tumor from his back. The surgical operation was just begun, when the patient was seized with a fit of laughing which continued nearly a minute. When quieted, a little more amylene was administered. At the close of the operation, his respiration became embarrassed, his pulse feeble, and on trying to wake him, it proved to be too late; the asphyxia was complete. It was naturally inferred that the patient had breathed too much amylene. According to Dr. Snow, the air which a patient respires ought not to contain more than 15 p. c. of amylene, just as it should not contain over 5 p. c. of chloroform; and he holds that with a measured quantity there would have been no accident. The recently invented apparatus of Dr. Heurteloup meets precisely this case, and offers a new method of operation.

Anesthesis by "Projection."—To avoid the accidents due to excessive inhalation of the anesthetic agent, and especially to insure that the material should be mixed with the requisite amount of air, Dr. Heurteloup, known in surgery for the invention of Lithotripsy, has contrived an apparatus for this end, (having in his experiments made use of chloroform). The apparatus is like a syringe with a small bellows for throwing in air in place of the piston, and having a gauze partition on which the chloroform is poured. The working of the bellows throws a stream out of the small end in a jet, which contains more or less chloroform, according as the discharging tube is brought more or less near to the bellows. The jet is established only on working the bellows, and there is no waste of chloroform during the operation.

Compressed air.—It is well known that in the construction of the Franco-Italian railroad it is necessary to tunnel Mount Cenis through a distance of several kilometers in length. To supply the air needed for the workmen for respiration, it has been proposed to use powerful pressure. But no means of accomplishing this result have been devised, and the project of the tunnel is suspended. It is found that an increase in the length of a tube connected with a blowing machine diminishes, at a rapid rate, the intensity of the movement. This calls to mind the experiment of Wilkinson, who established the fact of the resistance exerted by the walls of a pipe on the velocity of the compressed air. M. Daru, engineer on the Northern railroad, has added the following facts meriting consideration. A wheel which made thirty revolutions a minute in driving a blast through straight pipes one meter long, made only twelve when the pipes were four meters long and were bent twice at an angle, one of them right and the other very obtuse. When, on taking the air from close at hand the effect produced was great, it was very feeble when it came through pipes ten meters long with a right angled bend.

In the experiments by Wilkinson, the compressed air ceased to be transmitted at 280 meters; and it is not 280 meters, but a length of several kilometers, which must be met at the tunnel of Mount Cenis. The question therefore is far from being resolved.

Artificial meadows; Cultivation of Madder.—In speaking in a former communication of the products of Algeria, we alluded to the value of the Madder (Rubia tinctorum) cultivated in that country. It has been found there that a plantation of madder may be used as pasturage for cattle for several years, without the roots, at the end of this time, losing any of their tinctorial qualities.

In March, 1851, M. Peyre, a farmer of Oran, sowed a quantity of madder in a field well prepared, the soil of which was strong and argillaceous. It was left without care and after the first year, through the years 1852, 1853, 1854, it was free to cattle, who found there abundant pasturage during the season of great heat. At the end of this time, the roots were pulled up and submitted to the examination of competent men; and they proved to be of excellent quality, even rivaling the best of French madder.

From these observations it results, that we may make, without great expense, artificial meadows on land deprived of any means of irrigation, and derive a crop of madder having all its coloring principle preserved.

Toxicology.—Researches on Arsenic.—Dr. Blondlot of Nancy has just observed a fact which explains the contradictions encountered by inexperienced chemists in attempts to detect arsenic in connection with organic matters. It is this:—that when sub-

stances poisoned have been left to putrefy, some sulphuret of arsenic is formed at the expense of the sulphuretted hydrogen, and this, as is well known, escapes detection by Marsh's apparatus. Sulphuret of arsenic also forms when the suspected matters are carbonized by the action of sulphuric acid after the process of Flandin and Danger. The sulphuret of arsenic may be extracted by washing the carbonized mass with ammonia; this dissolves the sulphuret; then convert the arsenic into arsenic acid (AsO^5) by means of boiling nitric acid, so as to obtain a second solution; this, added to the first, may then be tested in Marsh's apparatus.

Aquarium.—The aquarium has already become a common source of amusement and instruction. The cultivation of water plants, as the Victoria regia, and experiments in pisciculture have contributed to this result; and also, the researches of Mr. Warrington, on what is called the organic equilibrium for waters, so that the water in a globe, by a proper selection of kinds of animals and plants, may be kept pure and wholesome for the species without changing it.

The first idea of such aquatic reservoirs is attributed in the Cosmos to Mrs. Power, a lady of French descent but English by marriage, known to naturalists for her important researches on molluscs.

In 1832, Mrs. Power took up her residence on the coast of Sicily in order to study the molluscs and other marine animals. She remained there ten years and made two aquaria, one for molluscs without shells and the other for those with shells. The bottom of the aquaria was covered with sand, stones with seaweeds attached, branches of corals, star fish, different crustaceans and some small fishes, while her principal object of study was the *Argonauta Argo*. Situated on the coast she could readily change the water of the aquaria; the plan since developed by Mr. Warrington was not then understood.

Mrs. Power also used marine cages, called in Italy "Gabiole alla Power," in England "Power cages," which she had constructed in the lazaretto of Messina. Stones with Algæ, and corals with adhering shells were introduced, and then, the Argonauta, Echini, fishes, etc. The feeding of the animals was attended to twice a day. A staging was erected just above the water's surface, where the cage could be raised near the surface, and the animals conveniently examined; and there Mrs. Power sat during the long hours carrying on her observations, witnessing the Polypus or Cephalopod of the Argonaut mending its shell, and studying the habits of many other species.

Mrs. Power also constructed a portable aquarium for studying small molluscs. Her researches were continued through fifteen years, and many important results were contributed by her to the progress of science.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Electrolytic investigations.*—Under this title Magnus has published an elaborate and important memoir reviewing the results of previous investigations and containing many new and important facts. We shall here give only the author's summary, referring for the details to the original memoir.

(1.) To explain the so-called double decomposition observed by Daniell and Miller, it is not necessary to assume an oxysulphion, oxynitrion, &c. This assumption is refuted by the fact that compounds like S+4O, N+6O are never separated at the positive electrode. It is true that at this electrode a full equivalent of oxygen, corresponding to the metal separated, is found, but of the acid there is only a portion, frequently only 60 per cent. By employing a porous diaphragm the remainder of the acid is found in the negative cell.

(2.) When several salts are present in the same solution, the current at a certain intensity decomposes only one of them. In like manner when a salt dissolved in water is used as an electrolyte, with a certain force of the current only the salt, and not the water, is decomposed. There is therefore for every compound electrolyte a limit of intensity, at which only one of its constituents is decomposed.

(3.) When currents are employed the intensity of which is less than the limit, the whole quantity of electricity passes over to the substance to which this intensity relates. This substance alone is decomposed. The limit itself therefore corresponds to the maximum of electricity which can pass to this substance, or to the maximum of the substance which can be decomposed in a given time with unchanged electrolytes and unchanged electrodes.

(4.) This limit depends upon the size of the electrodes; on the decomposability of the different constituents of the electrolyte; and on the relative quantity in which they exist in it.

(5.) Since, in the application of the same intensity, the electrodes may be nearer to or farther from each other, the maximum of the better conducting substance which is decomposed by the same current and the same electrodes is the same, whether the electrodes are nearer to or farther from each other.

(6.) The limit of the intensity is proportional to the magnitude of the electrodes, provided that this section of the electrolyte is the same as the size of the electrodes. This proportionality holds good however only so long as the constitution of the electrolytes remains unchanged.

(7.) The conduction of electricity through an electrolyte, and the decomposition which takes place thereby, may be referred to the case of the induction of electricity upon insulated conductors.

(8.) In this manner the difficulty of the so-called double decomposition raised by Daniell may be overcome.

(9.) It requires the same force to separate a simple substance from a binary combination which is necessary to separate it from a more complex saline compound.

(10.) The same force is requisite to separate the same quantity of chlorine from the protochlorids as from the perchlorids of tin and copper. But we obtain in this way from the protochlorids twice as much metal with the same current as from the perchlorids.

(11.) The same force is also necessary to obtain equal quantities of oxygen from a solution of iodic acid and from dilute sulphuric acid which are decomposed in separate vessels. In this case however only one-fifth of an equivalent of iodine is obtained for one equivalent of hydrogen separated from the sulphuric acid.

(12.) Faraday's law is applicable in its fullest extent inasmuch as equivalent quantities are always separated from complex saline compounds. But the galvanic are not the same as the chemical equivalents.

(13.) Saline particles change their position in eletrolytes partly by continual decompositions and recombinations, partly by diffusion. The density of the solution exerts a sensible influence upon the diffusion, which is however different in different saline solutions.—*Ann. der Physik und Chemie*, cii, 52.

2. *On the influence which metals exert upon radiant heat.*—KNOBLAUCH has communicated a memoir upon this subject the most important results of which in the author's own words are as follows.

(1.) Metals like gold, silver, and platinum in thin plates are to be regarded as diathermanous bodies, which allow a portion of the rays of heat to pass through, which portion diminishes more and more with increasing thickness of the metal.

In this transmission certain metals, for example gold and silver, exert an elective absorption upon the rays of heat analogous to that of transparent colored bodies upon rays of light; while others, like platinum, partly absorb and partly transmit all kinds of rays of heat in an equal degree, as is the case with transparent colorless bodies with respect to light.

Rays of heat, according to the foregoing statement, exhibit, after their passage through the metals of the first class, different relations, with respect for instance to their passage through diathermanous bodies, from those which they show before their entrance into these, and this peculiarity is expressed more distinctly in proportion as the metallic layer passed through is thicker. In metals like platinum this thickness exerts no influence on the quality of transmitted heat.

These last would behave like gray substances with respect to the rays of heat as well as toward the visible rays. Substances which can be compared with those which are transparent and white with respect to lights, appear not to exist for radiant heat.

(2.) In the case also of diffuse reflection, certain metals, as for instance gold, silver, mercury, copper and brass, like opaque colored bodies with respect to light, exhibit an electric absorption with regard to radiant heat, in consequence of which this becomes changed in its properties. Others on the contrary, platinum, iron, tin, zinc, lead, alloys of lead and tin, and german silver, reflect all kinds of rays of heat in equal proportion, exactly as colorless opaque bodies reflect lights.

The last gray metals behave similarly with respect to heating and visible rays. No bodies are known which act upon radiant heat as opaque

white bodies act upon light. The peculiarities which distinguish rays of heat reflected from metals from those which are not reflected, with respect for instance to their capacity to pass through diathermanous bodies, depend upon the nature of the source of heat to such a degree that differences which are strikingly marked in the case of the sun's heat, are diminished in the rays of a Locatelli lamp, and disappear completely with the heat of a dark heated metallic cylinder.

The quality of the metallic surface according as it determines a diffuse or a regular reflection has such an influence as either to permit the differences mentioned to be exhibited to their full extent, or again to disappear to such a degree that the rays are not to be distinguished from each other before and after reflection.

The same is true of a change in the angle of incidence. As this gradually permits the diffuse reflection of a rough metallic plate to pass into regular reflection, under a constantly increasing intensity, as the rays become more and more obliquely incident, so it also diminishes the differences between the reflected and non-reflected heat, till both at last exhibit a perfectly similar quality.—*Ann. der Physik und Chemie*, ci, 212.

3. *On an optical test for Didymium*—GLADSTONE has discovered in the spectrum produced by solutions of didymium two well marked black lines; one in the yellow directly following the bright space immediately beyond the fixed line *D*, the other in the green midway between *E* and *b*. The extreme blackness of the first of these lines even in thin strata of liquid renders its existence a very delicate and valuable indication of the presence of didymium. The salts of lanthanum and cerium produce no similar effects. One part of sulphate of didymium dissolved in 1000 parts of water showed the line in the yellow as a distinct darkening, when half an inch of the solution was looked through. The presence of other bodies does not interfere with the application of the test, so far at least as other metallic solutions have been studied.—*Quarterly Journal of the Chemical Society*, No. xxxix, 219.

4. *On the employment of the salts of alumina in the analysis of plants.*—ROCHLEDER has pointed out the superiority of alumina over hydrate of oxyd of lead for the separation of the proximate constituents of plants. The author in the first place remarks that organic substances may be divided into two classes with reference to their behavior toward alumina. Many coloring matters, as well as other substances, are precipitated by alumina from their solutions, while others on the contrary are not affected. Alumina therefore gives us a method of separating the one class from the other. The precipitates are less gelatinous than alumina and more easily washed out. In many cases a solution of alum may be added directly to the extract of the plant and the alumina then precipitated in combination with the organic matter by means of ammonia. As an example of the method, an aqueous decoction of horse-chestnut bark, treated with a solution of alum and then with ammonia, gives a fawn-colored precipitate. The filtered solution is wine-yellow. The solution neutralized with acetic acid and evaporated to dryness in a water-bath gives a mass containing the sulphates of potash and ammonium, a little acetate of ammonia and all the æsculin. This may be separated by boiling with a little strong alcohol and filtering. The æsculin crystallizes on evaporation and after a

single recrystallization is perfectly pure. The tannic acid is easily separated from the fawn-colored precipitate by solution in water containing acetic acid, filtration, precipitation with a salt of lead, and decomposition of the lead salt with sulphydric acid. In conclusion the author suggests that the employment of the hydrate of alumina will permit us to prepare many substances at a cheap rate which have hitherto found no application in consequence of their high price.—*Sitzungs berichte der k. k. Acad. zu Wien*, xxii, *quoted in Journal für prakt. Chemie*, 71, p. 414.

5. *On some derivatives of gallic acid.*—NACHBAUR has instituted at the suggestion of Prof. Hlasiwetz and under his direction, a series of experiments to determine whether ternary radicals can be introduced into the molecule of gallic acid. The process employed consisted in heating gallic acid with the chlorids of the radicals, acetyl, butyryl, &c. The author describes four substituted acids, the names and formulas of which are as follows:—

Tetracetyl-gallic acid, $C_{14}\left\{\begin{matrix}(C_4H_3O_2)_4\\H_2\end{matrix}\right\}O_{10}$,

Triacetyl-gallic acid, $C_{14}\left\{\begin{matrix}(C_4H_3O_2)_3\\H_3\end{matrix}\right\}O_{10}$,

Dibütyryl-gallic acid, $C_{14}\left\{\begin{matrix}(C_8H_7O_2)_2\\H_4\end{matrix}\right\}O_{10}$,

Dibenzoyl-gallic acid, $C_{14}\left\{\begin{matrix}(C_{14}H_5O_2)_2\\H_2\end{matrix}\right\}O_{10}$.

—*Chemisches Central Blatt*, No. 47, p. 740.

6. *On the combinations of tartaric acid with saccharine matters.*—In pursuing his investigations into the compounds formed by saccharine matters, Berthelot has been led to produce acid compounds of a peculiar nature with mannite, dulcine, and glucose. These compounds establish the constitution of a great number of natural compounds analogous to tannin, and capable, by taking up water, of splitting into glucose and a corresponding acid. In the present communication the author points out the combinations of tartaric acid with glucose, milk sugar, cane sugar, sorbine, pinite, quercite, and erythroglucine, as well as a compound of glucose and citric acid. All these bodies may be prepared and purified by the following process. Equal weights of tartaric acid and saccharine matter are mixed intimately and heated for a day or two in an open vessel to a temperature of 120° C. The cooled mass is rubbed with a little water and carbonate of lime and filtered. The filtrate contains the lime salt of the new acid, mixed with the excess of saccharine matter; it is precipitated by twice its volume of common alcohol, and the precipitate washed with alcohol and diluted with an equal volume of water. The lime salt is again dissolved in water and again precipitated as before, and these operations several times repeated. From the purified salt the acid may be separated by oxalic acid. The author represents the reactions which result in the formation of the new acids by the simplest possible formulas representing the ratio of the bodies concerned. These formulas show that, as in the case of alcohol in the sulphovinates, the saccharine body minus a certain quantity of water, replaces in the acid a portion of the base necessary to saturate this acid in the isolated state. It is probable that, as in the case of the compounds of glycerine, the

same sugar may form many compounds with tartaric acid. The author describes only those which he has obtained. For the formulas we must refer to the original paper.—*Comptes Rendus*, xlv, 268.

[*Note.*—From the above it will be seen that we owe to Berthelot the discovery of the true constitution of three entire series of organic bodies, viz., the glycerids or fatty bodies; the sugars and their congeners; and the glucosids or acid and neutral bodies which split into sugar and other acid or neutral bodies by boiling with acids, alkalies or water.]

7. *On the action of light upon oxalate of peroxyd of iron.*—DRAPER has communicated a paper on the measurement of the chemical action of light which, together with many interesting remarks and suggestions, contains a notice of the action of light upon the oxalate of peroxyd of iron. The golden yellow solution of this salt undergoes no change when kept in total darkness, but on exposure to a lamp or to daylight is decomposed with evolution of carbonic acid and precipitation of oxalate of the protoxyd as a lemon-yellow powder. In sunlight it effervesces violently. The indigo ray is especially active in producing this effect and undergoes absorption in doing so, since a sunbeam which has passed through one layer of solution is incapable of affecting another. The author points out several methods of employing this salt in photometry, the most advantageous of which is to collect and measure the quantity of carbonic acid absorbed in a given time. The solution is sufficiently sensitive for all ordinary purposes. When great sensitiveness is required the author recommends the use of the tithonometer invented by him in 1843 and since employed in a modified form by Bunsen and Roscoe.—*L. and E. Phil. Mag.*, Sept. 1847, No. 92, p. 161. W. G.

8. *On the Chemistry of the Primeval Earth;* by T. STERRY HUNT. (Extract of a letter to Prof. J. D. Dana, dated Montreal, Nov. 25, 1857.)—The primitive rocks which filled so large a place in the geological systems of the last century are now being forgotten. We have learned that the oldest visible portions of the earth's crust are made up of sediments, the ruins of still older rocks, which were as varied in their characters as are their derivatives. The primeval substratum has thus constantly receded before advancing science, and we are led to the conclusion that mechanical and chemical conditions similar to those of the present epoch presided over the formation of the most ancient rocks known.

But although the *materia prima* of the sedimentary rocks has long since been buried beneath its own ruins, its nature offers an interesting subject of consideration to the chemical geologist. If we admit the igneous theory of the earth, we may obtain a conception of the nature of the once liquid globe and of its atmosphere, by supposing the now existing matters of the earth's crust and the surrounding fluids to be made to react upon one another under the influence of an intense heat. The quartz would decompose the carbonate of lime with the production of a silicate and the liberation of carbonic acid, whose volume would be farther augmented by the combustion of all the mineral carbon at the expense of the atmospheric oxygen. The reaction between quartz and the chlorids of the sea, in the presence of aqueous vapor, would result in the formation of silicates and hydrochloric acid, while the sulphur would likewise be liberated as a volatile acid.

From these reactions there would result on the one hand a more or less homogeneous mass of silicates of alumina and alkalies, with silicates of lime, magnesia and iron, a mixture probably resembling dolerite, while the atmosphere would be made up of watery vapor, nitrogen, a probable excess of oxygen, with carbonic, sulphuric and hydrochloric acids representing all the carbon, sulphur and chlorine of the globe.

When the cooling of the globe had so far advanced as to allow of the precipitation of water from this dense atmosphere it would descend as an acid rain, which attacking, at an elevated temperature, the silicates, would give rise to chlorids of calcium, magnesium and sodium, mingled with sulphates of these bases. The liberated silica would probably separate during the cooling of the heated waters in the form of quartz.

The subsequent decomposition of the exposed portion of the primeval crust would result in the conversion of its feldspar into kaolin, and a soluble alkaline silicate, which decomposed by excess of carbonic acid would be carried to the sea as a bicarbonate, where decomposing the lime salt, it would give rise to chlorid of sodium and bicarbonate of lime, which would be partly precipitated in a crystalline form and partly secreted by marine animals. The carbonates of lime and magnesia set free during the slow decomposition of the primitive rock would also go to augment the proportion of carbonates in the ocean, and help to fix in mineral masses the carbonic acid of the atmosphere.

At length we reach the Carboniferous period of the earth's history, when a luxuriant vegetation completed the work of purifying the atmosphere, by transforming, as Brongniart long since suggested, the remaining excess of carbonic acid into carbon and oxygen gas, thus preparing the air for the support of warm-blooded animals.

By this hypothesis I think we get a clear conception of the generation from a primeval homogeneous mass of the quartzose, argillaceous and calcareous materials which make up the great bulk of the stratified rocks, and we obtain at the same time a notion of the origin of the saline constituents of the sea. The chemistry of the ocean, the formation from its waters of gypsum, rock-salt and magnesian rocks, and the modes by which potash has been eliminated from it by marine vegetation, and apparently by the formation of glauconite, suggest many important subjects of inquiry which I reserve for another occasion.

The history of our globe, especially during the time when chemical forces were yet in the ascendant, and were preparing it for the reign of organic life, offers considerations of great interest which I hope soon to be able to develop at greater length than I have done in these few lines.

9. *On the Amount and Frequency of the Magnetic Disturbances and of the Aurora at Point Barrow, on the Shores of the Polar Sea;* by Major General SABINE (Proc. Brit. Assoc., Athen., No. 1559).—Point Barrow is the most northern cape of that part of the American continent which lies between Behring's Strait and the Makenzie River. It was the station of H. M. S. Plover from the summer of 1852 to the summer of 1854, and to the Captain, Maguire, now in the Section, and officers of that ship, they were indebted for the very valuable series of observations which he was now about to lay before the Section, and in part discuss. They were furnished with supplies of provisions, &c. for Sir John Frank-

lin's ships, had they succeeded in making their way through the land-locked and ice-encumbered channel, through which they sought to effect a passage from the Atlantic to the Pacific. In this most dreary and otherwise uninteresting abode, Capt. Maguire and his officers happily found occupation during seventeen months, unremittingly, in observing and recording every hour the variations of the magnetic and concomitant natural phenomena, in a locality perhaps one of the most important on the globe for such investigations. Their observatory, placed on the sand of the shore, which for a long tract nowhere rose much over five feet above the sea, was constructed of slabs of ice, and lined throughout with seal-skins. The instruments had been supplied by the Woolwich establishment, with the requisite instructions for their use; and the observations were made and recorded precisely in the same manner as those of the Colonial magnetic observatories. These were sent by Capt. Maguire to the Admiralty, and were in due course transmitted to General Sabine, by whom they were subjected to the same processes of reduction as those made in the Colonial observatories.

The author then exhibited to the Section six long rolls, containing the results of this discussion, giving the reduced observations at each of the hours of the twenty-four. A sufficient body of the larger disturbances having been separated from the rest, it was found at Point Barrow as elsewhere, wherever similar investigations had been made, that in regard to the frequency of their occurrence, and the average amounts of easterly and westerly deflections, the disturbances followed systematic laws depending on the hours of solar time. The laws of the easterly and westerly were also found at Point Barrow, as elsewhere, to be distinct and dissimilar. The author explained how these observations, which manifestly related to those arising from what were called "storm," were separated from the rest; and when that separation was effected, the law of the true solar variation was shown distinctly to be observed. But upon instituting a comparison between the disturbance laws at Point Barrow and Toronto, it was found that the laws of the deflections of the same name at the two stations did not correspond; but, on the other hand, there existed a very striking and remarkable correspondence between the law observed by the easterly at Point Barrow and the westerly at Toronto, and between the law of the westerly at Point Barrow and easterly at Toronto; and this correspondence was shown to exist not in slight or occasional particulars only, but throughout all the hours in well-marked characteristics of both classes of phenomena; and it follows from the correspondence in the hours at which opposite disturbance deflections prevail, that the portion of the diurnal variation which depends upon the disturbances, has opposite, or nearly opposite, characteristics at the two stations. The importance of eliminating these disturbances from the regular march of the solar variation was then pointed out in both: for when the diurnal variation is derived from the whole body of observations at Point Barrow, retaining the disturbances, the westerly extreme of the diurnal excursion, which, as is well known, occurs generally in the extra-tropical part of the northern hemisphere a little after 1 P.M., is found to take place at 11 P.M.; but when these larger disturbances are omitted, the westerly extreme falls at the same time as elsewhere—viz.,

1 P. M.; and the author suggested the probability that the anomalies which have sometimes been supposed to exist in the turning hours of the solar diurnal variation in high latitudes may be susceptible of a similar explanation. It appears, then, by a comparison of the Point Barrow and Toronto observations, that in the regular solar diurnal variation the progression at the two stations is similar, the easterly and westerly extremes being each reached nearly at the same hours, whilst in the disturbance diurnal variation this progression is reversed. Another distinction exists in their magnitudes, which is found in the solar diurnal variation to be as nearly as may be in the inverse ratio of the values of the horizontal force at the two stations, (which is the antagonistic force opposing all magnetic variations,) whilst on the other hand the increase in the range of the disturbance variation is many times greater than it would be according to the same proportion. It would appear, therefore, that the absolute disturbing force must be greater at Point Barrow that at Toronto.

The author then proceeded to point out the concomitant occurrences of the auroral manifestations. The observers noted at each hour whether or not there was an auroral display: from 11 A. M. to 3 P. M. no auroral displays were ever observed; but the number of them was found progressively to increase from 3 P. M. to 1 A. M., and then again in regular progression to decrease to 0, at 11 A. M. The frequency of the occurrence of the aurora may be judged of, when it is said that during six months, —December, January and February of 1852–53, and the same of 1853–54,—the aurora was seen six days out of every seven. The hour of the day at which no auroral display is ever observed corresponds with the minimum of westerly disturbance, while the maximum of both is found at the same hour of westerly disturbance—viz., 1 A. M. The frequency of the aurora, also, and the amount of westerly deflection of the magnet also accord; whilst on the other hand the auroral hours appear to have little or nothing in common with the turning hours or the progression of the easterly deflection.

When Sir John Franklin was going out on the expedition which deprived his country of the invaluable services of himself and his brave companions, he had been furnished by the Admiralty both with instruments carefully adjusted and compared with standards, and with full instructions for their use, and for the making and recording hourly observations of the utmost importance in the several stations he might occupy in these seas; and in the last letter which had ever been received from him, he had expressed his determination to put up those instruments at the several stations at which he should winter. Now when his ardor in these pursuits and that of Capt. Crozier, the second in command, and the other officers, were taken into account, there could remain no doubt that such observations had been made and recorded, and that these records still existed in some of the places he had last been in. When he (General Sabine) was with Capt. Parry, in 1818, they had made observations with the pendulum for determining the figure of the earth, and others of great scientific importance, on their way towards Behring's Straits. They had been exposed to considerable risk of the ships being lost, and were about to take to the boats and proceed overland, and in preparation for this merely prepared to carry with them abstracts of the observations, leaving

the original full records safely deposited in secure cases in the cabins of the ships, to be found by those who doubtless would be sent out to look for them. He had, therefore, no doubt that if the ships of Sir John Franklin were still in existence, in their cabins were to be found those scientific treasures; and this was one of the reasons why men of science were so anxious to have the ships carefully looked for, and it was a sacred duty even to the memories of those who had sacrificed their lives in procuring such results to do them the justice and honor of having them recovered if possible.

At the conclusion of General Sabine's address, the President requested Capt. MAGUIRE to favor the Section with a portion of what he had observed in these most inhospitable, but, to the scientific inquirer, deeply interesting regions. Capt. Maguire, with that modesty so characteristic of the British sailor, disclaimed for himself much merit, and assigned all the praise of making and recording these hourly observations, through such a very lengthened period, to his brother officers, he himself only occasionally helping, particularly when he was out with exploring parties. He said he much wished he could convey to the Section any vivid impression of the beauty and brilliancy of the auroral displays in those regions. It was never seen during the hours of daylight, or those hours which corresponded to mid-day, but towards evening its displays began, at first towards the north; it then extended in splendid arches spanning the entire sky, and seeming to end in beautiful coronæ towards the zenith; these were occasionally of the most brilliant and varied tints and colors. It spread gradually more south, and at length died away towards the morning hours in the south. Such were the beauty and interest o. these displays, that men and officers constantly, with the thermometer at and below 40° below zero, stood out for hours witnessing the glorious scene. During these auroral displays he could not say that he had ever witnessed those violent agitations of the needle that others had described, but the easterly disturbance of the variation seemed to be simultaneous with its northerly display, and the westerly to its influence when it had passed to the south. At some distance from the ships, say about five miles, the water shoaled, and the ice had been driven up into beautiful rocky pinnacles; beyond this, again, the water was always free of ice, and its temperature was frequently found to be 28° above zero, when that of the air above was even 40° below zero; the consequence was, that it had all the appearance of a boiling sea, so great was the quantity of vapor thrown up from it.

Admiral FITZ-ROY, Mr. GASSIOT, and other members of the Section, spoke of the most important interest which these inquiries had in a scientific point of view, and could not help thinking that if the Admiralty had been more strongly pressed upon the subject, they would not have persevered in declining to aid in the expedition which had gone out this year.

10. *On the Direction of Gravity at the Earth's Surface;* by Prof. HENNESSY (Proc. Brit. Assoc., Athen., No. 1559).—If the earth's surface be considered to coincide with that of the liquid which covers three-fourths of the entire spheroid, gravity should be considered as perpendicular to it at every point. If, however, the earth were stripped of all its seas and

oceans, the surface would present considerable inequalities. From what is now known regarding the depth of the ocean, the continents would appear as plateaus elevated above the oceanic depressions to an amount which, although small compared to the earth's radius, would be considerable when compared to its outswelling at the equator, and its flattening towards the poles. The surface thus presented would be the true surface of the earth, and would not be perpendicular to gravity. If a kind of mean surface be conceived intersecting this, so as to leave equal volumes above of elevations, and of depressions below it, it is not allowable to assume that such a surface is perpendicular to gravity. The mean surface of the solid crust of the earth would not be perpendicular to gravity, if, after the process of solidification had commenced, any extensive changes in the distribution of matter in the earth's interior could take place. If the fluid matter in solidifying underwent no change of volume, the forms of the strata of equal density within the earth would be the same at every stage of its solidification. But if, as observation indicates, such fused matter, on passing to the solid crystalline state, should diminish in volume, the pressure on the remaining strata of the fluid would be relieved, and they would tend to assume a greater ellipticity than they had when existing under a greater pressure. The general result of this action would manifestly be to produce a change in the direction of the attractive forces at the outer surface of the solid crust. The direction of a plumb-line would be slightly altered so as slightly to increase the apparent latitudes of places over a zone intermediate between the equator and poles.

M. D'Abbadie stated several cases which he had met with, where monuments existed which showed that the direction of gravity at some former period must have been very different in relation to these particular portions of the earth from what it now was. Other members also noticed deviations of the plumb-line from its normal position, and some of them which seemed to depend on the season of the year.

The President, Dr. Robinson, stated that he was the first to direct attention to those changes of level which depended on the season of the year. This he was led to observe from the fact, that the entire mass of rock and hill on which the Armagh Observatory was erected was found to be slightly, but to an astronomer quite perceptibly, tilted or canted at one season to the east, at another to the west. This he had at first attributed to the varying power of the sun's radiation to heat and expand the rock throughout the year; but he since has had reason to attribute it rather to the infiltration of water to the parts where the clayslate and limestone rocks meet. The varying quantity of this through the year he now believed exercised a powerful hydrostatic energy, by which the position of the rock was slightly varied.

II. MINERALOGY AND GEOLOGY.

1. *Brucite at Wood's Mine, Chester Co., Pennsylvania.*—In a letter from Dr. W. D. Hartmann of Westchester, Pa., he states that Brucite occurs at Wood's Mine in seams in serpentine from one to four inches in width. The mineral is broad foliated, folia several inches square being easily obtained, and either opaque silvery white or translucent to transparent. Occasionally it has a fine rose tint. The vein has an outer layer of a greenish chlorite-like mineral.

2. *Descriptions of New Species of Palæozoic Fossils from the Lower Helderberg, Oriskany Sandstone, Upper Helderberg, Hamilton and Chemung Groups;* by JAMES HALL. 146 pp. with wood-cuts. Extracted from the Report of the Regents of the University. Albany, 1857.—This pamphlet is issued by the State in advance of the third and fourth quarto volumes on the Palæontology by Prof. Hall, of which the third is now nearly ready. It contains a large number of new species, though but a part of what those volumes will embrace. Prof. Hall here adopts Suess's genus Meganteris, and the *Atrypa elongata* of Conrad characteristic of the Oriskany Sandstone (*Terebratula ovoides* of Eaton in his Geol. Text-book, 1832), is named the *Meganteris ovoides;* and the *Pentamerus elongatus* of Conrad from the Onondaga limestone, is the *Meganteris elongatus.*

The pamphlet closes with a short paper by Mr. Hall on his genus Tellinomya (see Pal. N. Y., vol. i). The paper was published originally in the "Canadian Naturalist and Geologist." From recently discovered specimens, he has found that the teeth of the hinge have a close relation to those of the genus Nucula, and he is enabled to give the following corrected description:—

TELLINOMYA.—Shell equivalve, equilateral or subequilateral, closed, smooth or marked by lines of growth, ligament external; hinge-line curved, sometimes subangular, with a continuous series of small curved transverse teeth, which diminish from the extremities to the beak, beneath which, they are much smaller. Muscular impressions double, two anterior and two posterior, one large and strongly impressed, the other smaller, lying above and between the larger one and the hinge-line; pallial impression simple.

He refers to it, *Ctenodonta* of Salter.

3. *Cosmogony, or the Mysteries of Creation;* by THOS. A. DAVIES. 416 pp., 8vo. New York.—There is quite an extensive show of science through this large volume, but by one ludicrously ignorant of its first principles and correspondingly presumptuous. The following are some of the sage conclusions argued out from the author's scientific materials: That the theory that boulders or rounded stones are water-worn rocks "is entirely untenable;"—that all "vegetable mould was made undoubtedly in connection with the vegetable kingdom in the primitive creation," and that we may as well say milk comes from granite as ingredients that improve or make soils;—that fossil bones and shells, ancient sea beaches and ripple marks, etc., are primitive creations, spoken into existence as they are, as types of future existences;—that the coal of all coal beds "must be the slow but mysterious carbonization of the clay slates!" Similar views (the *last* excepted) in the biblical scholar not claiming any knowledge of science should have respectful consideration; but not so, when the product of scientific pretense.

We have quoted a few of the surface ideas only of the work. Its philosophy makes profound plunges; and one of the jewels of knowledge gathered from the mud, to which a page of capitals is wholly devoted, is the order in which the laws of matter were established in the course of the first four days of creation:—namely, Attraction of Gravitation *after* Form, Color, Electric Attraction, Cohesive Attraction, Endosmosis and

Exosmosis; Electric attraction, etc. *after* Chemical affinity; Chemical affinity *after* Aggregated Existence; and Motion and Equilibrium *last.*

The author is to be commended for his desire to sustain sacred truth; but he has made a stupid book that will damage the cause.

4. *On the Existence of Forces capable of changing the Sea level during different Geological Epochs;* by Prof. HENNESSY (Proc. Brit. Assoc., Athen., No. 1559).—If, in assuming its present state from an anterior condition of entire fluidity, the matter composing the crust of the earth underwent no change of volume, the direction of gravity at the earth's surface would remain unchanged, and consequently the general figure of the liquid coating of our planet. If, on the contrary, as we have reason to believe, a change of volume should accompany the change of state of the materials of the earth from fluidity to solidity, the mean depth of the ocean would undergo gradual though small changes over its entire extent at successive geological epochs. This result is easily deduced from the general views contained in other writings of the author, whence it appears, that if the surface stratum of the internal fluid nucleus of the earth should contract when passing to the solid state, a tendency would exist to increase the ellipticity of the liquid covering of the outer surface of the crust. A very small change of ellipticity would suffice to lay bare or submerge extensive tracts of the globe. If, for example, the mean ellipticity of the ocean increased from $\frac{1}{300}$ to $\frac{1}{299}$, the level of the sea would be raised at the equator by about 228 feet, while under the parallel of 52° it would be depressed by 196 feet. Shallow seas and banks in the latitudes of the British isles, and between them and the pole, would thus be converted into dry land, while low-lying plains and islands near the equator would be submerged. If similar phenomena occurred during early periods of geological history, they would manifestly influence the distribution of land and water during these periods, and with such a direction of the forces as that referred to, they would tend to increase the proportion of land in the polar and temperate regions of the earth, as compared with the equatorial regions during successive geological epochs. Such maps as those published by Sir Charles Lyell on the distribution of land and water in Europe during the tertiary period, and those of M. Elie de Beaumont, contained in Beudant's 'Geology,' would, if sufficiently extended, assist in verifying or disproving these views.

III. BOTANY AND ZOOLOGY.

1. *Monographie de la Famille des Urticées;* par H. A. WEDDELL (Archives du Muséum, tom. ix, livr. 1–4), 4to, 1856–7.—Dr. Weddell's preliminary studies upon the proper *Urticaceæ* were published a few years ago in the *Annales des Sciences Naturelles.* Since then, botanists, aware from this and his other works that the subject was in most able hands, have been anxiously waiting for his full monograph. This, we understand, is now completed, although the last fasciculus has not yet reached this country. The greater part is before us, and an admirable monograph it is, worthy of a place in the *Archives* which contain that model one on the *Malpighiaceæ* of his lamented botanical master. It illustrates in detail about 470 species, under 40 genera, and is accompanied by 20 well-filled plates, drawn by the author. It opens with a Con-

spectus of the members of the great group to which the true *Urticaceæ* belong (which the author inclines to receive rather as the orders of a class than as suborders of an extensive order, fully admitting, however, their close affinity *inter se*), followed by a brief indication of the principal investigators of these plants, and of the resources at his own command. A general account of the organs of vegetation and reproduction, of the affinities, of the geographical distribution of the plants of the group, and of their properties and uses, conclude the preliminary matter. The body of the work is occupied by their systematic arrangement and description.

Apetalæ being viewed as degenerations of *Polypetalæ*, our author searches among the latter orders for the nearest relatives of the great Urticaceous order or alliance, and finds them in the *Tiliaceæ*, that is, in the group of orders of which the *Malvaceæ* are the highest development. According to Weddell's happy illustration, *Malvaceæ* crown the summit of a three sided pyramid, with *Sterculiaceæ*, *Byttneriaceæ*, and *Tiliaceæ* just below them, one upon each face; under the *Byttneriaceæ* he ranks the *Euphorbiaceæ* with the *Antidesmeæ*, and under these, at the very base of the pyramid, the *Scepaceæ*, the lowest degradation in this direction of the *Malvaceous* type. On the adjacent face, under the Tiliaceæ, and on the same level with the Euphorbiaceæ he inscribes the *Urticaceæ*, with the *Cupuliferæ* perhaps underneath them. Upon this ingenious plan of representation, the apetalous orders throughout may be most conveniently and instructively ranked under their superior types;—bearing in mind that some types degrade as much within an order (e. g. *Euphorbiaceæ*, *Onagraceæ* inclusive of *Haloragеæ*, *Caryophyllaceæ* including *Illecebreæ*) as others do through a series of two or three orders, or even as the same group does (e. g. *Caryophyllaceæ*) through a series of orders on the other sides of the pyramid.

The reason why this mode of representation will exhibit botanical affinities so well is, that (as we have elsewhere remarked) the vegetable kingdom does not culminate,—as the animal kingdom does,—and therefore offers no foundation whatever in nature for a lineal arrangement even of its great groups. But it would appear that the *Dicotyledonous* orders might be arranged under a considerable number of short series, in groups converging upon the most fully developed or representative order of each type, so as to exhibit what we now know of the system of nature much better than in any other way.

We think that Dr. Weddell's idea of the affinity of *Urticaceæ* is a good one. The floral and seminal characters, the true *criteria* of affinity are not abhorrent, but present some strong points of relationship, as do the organs of vegetation. These, once established, allow us to feel the force of the striking coincidence in the bast-tissue of the bark, so remarkable in all this alliance for the length, fineness, and toughness of the fibres, their union end to end, and their lateral independence, admirably adapting them for their use as textile materials, in which *Urticaceæ* vie with *Malvaceæ* and *Tiliaceæ*.

As to geographical distribution, Europe is very poor in *Urticeæ*, poorer even than would at first view be supposed, as the author remarks. For as nettles like an enriched soil, the five or six European species of *Urtica* and *Parietaria* so abound around habitations that they make up in the

multitude of individuals for the paucity of species, and perhaps cover nearly as much ground as the great number of intertropical species; two or three excepted, which also are weeds in the tropics. Temperate North America is not much richer in species than Europe. The greater part are found in the torrid zone, and in islands rather than continents; the Malay region, India, Mexico, and the West Indies together possess almost two-thirds of the known species.

Our remaining remarks shall be restricted to one well-known plant described in the work, and to another, of recent discovery, which unfortunately was not communicated in season to find a place in it.

The first is our common *Pilea pumila*. Dr. Weddell has overlooked the fact that Rafinesque had founded a genus (*Adice* or *Adike*) upon it, although the name is mentioned in the work, cited by him, where the plant was first published as a *Pilea*, and although Dr. Torrey had adopted Rafinesque's genus, and figured the species, in an earlier and more considerable work (*Flora of the State of New York*), which, having unfortunately been published by the State, and in a large edition, has in consequence remained almost unknown to science. Considering that the three sepals of the fertile flower in this species are nearly equal and not gibbous, it may be doubted whether the single species of Blume's genus *Achudemia*, differing only in having five sepals, should not rather be appended to *Pilea*. We dare say that Dr. Weddell would have so arranged it, if Blume had not published the genus.

Since the appearance of the third part of Weddell's monograph, but before it had reached this country, Dr. Torrey has published, in the Report on Dr. Bigelow's fine California collection made in Lieut. Whipple's Railroad Survey to the Pacific, a new Nettle, allied to *Bœhmeria* but with the penicillate stigma of *Urtica*, viz. his *Hesperocnide tenella* (Pacific R. R. Reports, 4, p. 139). This little plant, it now appears, comes nearest to Wight's monotypic genus *Chamæbaina* of India, of which better details than Wight's as to the female flowers and fruit are figured in the present monograph. The stigma is intermediate in character between that of *Chamæbaina* and that of *Urtica;* and, moreover, as the sepals of the male flower want the pointed gibbous tips of the former, the stipules are inconspicuous, and the cotyledons are not only reniform but (which is unnoticed in the published description) pretty strongly emarginate at the summit also, the genus will probably be retained.

Great thanks are due to Dr. Weddell for his labors upon this family, which he found in a most unsatisfactory and difficult state, and has left in such condition that Nettles and their allies are easy and inviting objects of study. Meanwhile, his other undertakings are carried on with spirit. Of his *Chloris Andina*, a flora of the higher Andes, three fasciculi have reached us since our former notice of the work, and we understand that the first volume is completed. In this work the author handles the *Compositæ* with marked ability. A. G.

2. *Miquel's Flora van Nederlandsch Indië*, or *Flora Indiæ Batavæ*,—for it has both Dutch and Latin title-pages, has made no small progress since our former notice of the work. Five parts (864 pages) have appeared of the first volume, which is devoted to the *Dicotyledones Polypetalæ* and *Apetalæ* conjoined; and one more fasciculus will apparently

complete this volume. The second, devoted to the *Monopetalæ* has extended to the third fasciculus. Thus far it is mainly occupied with the *Compositæ*, *Rubiaceæ*, *Apocyneæ* and *Asclepiadeæ*. Meanwhile the indefatigable author has issued two parts of the third volume, devoted to the *Monocotyledones*. The work appears to be faithfully elaborated, and must be highly useful to systematic botanists and creditable to the author.

A. G.

3. *Walpers: Annales Botanices Systematicæ.*—The second fasciculus of Dr. Mueller's continuation of this work has come to hand. It extends only from the *Nymphæaceæ* to the *Sterculiaceæ*,—at which rate a series of volumes will be required to bring up the arrears of scattered species published since the year 1850. A. G.

4. *Jahrbücher für Wissenschaftliche Botanik: herausg. von* Dr. N. Pringsheim. Berlin. Vol. I, part 1, 1857, large 8vo, pp. 138, with 10 plates.—This new work is to be devoted to original articles upon scientific botany in the strictest sense, and especially to the departments in which its editor is so distinguished, viz. Vegetable Anatomy and Morphology. The first article is by Dr. Pringsheim himself, one of a series of contributions to the Morphology and Classification of *Algæ*. It is a complete investigation of the morphology and development of the *Œdogonieæ* (*Œdogonium* and *Bulbochæte*) illustrated by six colored plates. The very important and curious results of Pringsheim's investigations upon the development and fructification of the lower Algæ, need to be presented in connexion with the contemporary ones of Cohn and Braun, which together have thrown new light upon this part of vegetable physiology, demonstrating that their reproduction is as truly sexual as that of higher plants and more directly comparable with that of animals.

The remainder of this fasciculus is occupied by New Researches upon the formation of the Embryo of Phanerogamous Plants, by Hofmeister, of Leipsic, with four plates:—one of them illustrating the impregnation and development of the embryo of a Balanophoraceous plant, *Cynomorium*. He thinks that in some cases traces of a cell-wall may be detected upon the germinal vesicle anterior to their fecundation by the pollen.

A. G.

5. *Radlkofer; on The Process of Fecundation in the Vegetable Kingdom, and its relation to that in the Animal Kingdom* (Leipsic, 1857, 8vo). Translated by Arthur Henfrey, F.R.S., &c., and published in the Annals and Magazine of Natural History for October and November, 1857.—This gives in English, and in an accessible form, a systematic and historical survey of the whole subject of vegetable fecundation, including the recent discoveries of Pringsheim, Cohn, Braun, and Bary, referred to in our previous article.

As to *Fungi* and *Lichenes*,—thanks to the observations of Itzigsohn upon the latter, and the most careful and persevering investigations of Tulasne upon both families,—the analogues of male organs in all probability are discovered, and their general presence recognized; but the fact of fecundation is not made out.

In the lower or green *Algæ*, fecundation was first *demonstrated* by Pringsheim. The "horns" of *Vaucheria* which Vaucher half a century ago observed and conjectured to be male organs, Pringsheim proved to

be so, having seen them open at the summit and emit a great number of free-moving corpuscles (spermatozoids), many of which found their way into the now open orifice of the protuberance which contains the forming spore, and were seen crowding against it, after which a membrane of cellulose appears over the surface of the mass of protoplasm and completes the spore. Whether one or more of the spermatozoids actually penetrates the protoplasm and so is included within the cell-membrane is uncertain; but Pringsheim thought it was the case, from having detected a colorless corpuscle like one of the spermatozoids inside of the membrane. Next Pringsheim demonstrated a similar fecundation in *Œdogonium*. His results, briefly published in the Proceedings of the Berlin Academy, and thence translated into French and English, are now given in detail in the first part of his *Jahrbücher*, noticed above. *Œdogonium* consists of a row of cylindrical cells. Some of these cells, usually shorter than the rest, become tumid, and, without conjugation, have their whole green contents transformed into a large spore. Pringsheim has ascertained that other cells of the same individual plant have their green contents transformed into a multitude of active corpuscles or zoospores, which, from their subsequent evolution and office, he names *androspores:* these escape by the opening of the mother cell, moving about freely by the vibration of a crown of cilia attached near the smaller end. One or more of these androspores fix themselves by the smaller end upon the surface of the cell in which a large ordinary spore is forming, or in the vicinity, and germinate there, growing longer and narrower at the point of attachment, while near the free end a cross partition forms, and sometimes another, making one or two small cells; this is the true antheridium; for in it a crowd of spermatozoids are formed, also endowed with motivity by means of vibratile cilia. Now the top of the antheridium falls off as a lid, the spermatozoids escape; the spore-cell at this time opens at the top; one of the spermatozoids enters the opening, its pointed end foremost; this becomes stationary upon or slightly penetrates the surface of the young spore, into which its contents are doubtless transferred, and a coat of cellulose is then, but not till then, deposited upon it, completing its organization as a spore, which in due time germinates, and grows directly into a plant like the parent.

But in *Bulbochæte*, and especially in *Sphæroplea*, so beautifully investigated by Cohn (see Ann. Sci. Nat., ser. 4, vol. 5), the spore does not directly develop into the normal or fruit-bearing plant. Instead of this, by an alternation of generations (to adopt that well-understood phrase), the spore proceeds to convert its contents by successive division into a large number of zoospores, different from the androspores, viz. small, oval or oblong bodies, furnished with two long cilia on a short beak at one end, and for a time moving actively about by their vibration. Coming to rest these zoospores germinate, by elongation and the formation of transverse partitions, into adult thread-like plants, consisting of a row of cells. In *Sphæroplea* the whole contents of the cells of some adult individuals condense into large green spores, as yet without a coat; while those of different individuals give rise to myriads of slender spermatozoids, moving by means of a pair of cilia fixed at the narrow end. The latter escape from the parent cell through a small perforation which now appears,

enter the spore-bearing cells of the fertile plant through a similar perforation in them, play around the spores, and at length one or more of them drives its pointed extremity into their naked surface; after which, fertilization being accomplished, a thick coat of cellulose is deposited to complete the spore. "Cohn does not consider that observations justify his assuming a direct penetration of the spermatozoids into the primordial spore-cell. It rather reemed to him as if they attached themselves on the outside of the spore, and were finally converted into mucilaginous globules."

Reproduction by conjugation of course had long been familiarly known in the lower *Algæ*. But it was questioned whether this was really analogous to sexual reproduction, since what appeared to be similar spores are often formed of the contents of a single cell without conjugation. Areschoug shows that these are abortive spores, incapable of germination; while those which result from actual conjugation will grow into new plants, without further metamorphosis, Vaucher's old observations to this effect having been confirmed by Braun and Pringsheim.

That in the *Fucaceæ* or olive-green *Algæ*, the large spores are fecundated by spermatozoids, produced in antheridia, was demonstrated by Thuret in the year 1850. And in more recent memoirs he has shown that the fertilization takes place through direct contact of the spermatozoids with the naked surface of the unimpregnated spore, then having only a protoplasmic coating; and that these spores will not develop nor hardly acquire a cell-wall unless so fertilized. His experiments upon diœcious species are perfectly decisive upon these points. He observed the lively spermatozoids playing over the surface of the still-naked spore, fix themselves to it by the ciliated end, apparently by one of the cilia, and at length come to rest in contact with it; but he could not detect any material penetration of them into the body of the spore. Pringsheim, confirming all Thuret's observations, also thinks that the spermatozoids actually penetrate the spore-mass; but there is no direct proof of it. Indeed Thuret, in a very recent article (in Ann. Sci. Nat., ser. 4, vol. 7, 1857,) indicates the grounds of Pringsheim's probable mistake. The most interesting point in this last article by Thuret relates to the suddenness with which the cell-membrane is formed on the spore of *Fucus* after the access of the spermatozoids and the accomplishment of the act of fecundation. In six or eight minutes traces of the formation of the membrane are recognizable upon a considerable number of the spores. In ten minutes the presence of a membrane, may be clearly made manifest by the application of chlorid of zinc. In an hour the membrane has acquired considerable firmness and thickness, and the presence of cellulose is revealed by the action of sulphuric acid and iodine: an hour later and the blue coloration under the test is decided.

In the higher *Cryptogamia* and in the *Phanerogamia*, Radlkofer's treatise, though interesting for the history, offers nothing new to our readers. In fact its date precluded it from containing much of what is referred to in the preceding paragraphs. But the subject is still to be continued. A. G.

6. *Natural History of the Spongiadæ.*—J. S. Bowerbank, Esq., of Highbury Grove, London,—eminent in this and related departments of

microscopical research,—is preparing a general work upon the Sponges, and is very desirous of obtaining specimens from this country. The following instructions for their collection and transportation are extracted from a circular issued by Mr. Bowerbank:—

"Sponges are exceedingly various in their external form and appearance: they are either massive, branching, fan-shaped, cup-shaped, or coating other substances, and are frequently parasitical on horny zoophytes or sea-weeds. In substance they are light and elastic, rigid, gelatinous, fleshy, and sometimes hard and stony, and are frequently very vividly colored; and they vary in size from the tenth of an inch in height or diameter, to several feet. They are procured by dredging in from two or three to several hundred fathoms deep; and they are found in considerable quantities attached to rocks or sea-weeds, &c., between high and low water marks, and in the line of sea-weeds and other matters thrown up by the sea at high water mark. In every case the more they contain of their fleshy or gelatinous matter the more valuable they are. They should never be washed in either salt or fresh water, and especially not in the latter, as it makes them hard and brittle. They should be dried as speedily as possible, either in a shaded, breezy place or in a slack oven, after having been well drained of salt water; and if attached to small stones or other substances they should be preserved in the attached state. They may be packed in boxes from one to three feet square, or, if longer, a partition may be put in; and the best packing is soft, dried sea-weeds that have not been washed in fresh water. The small sponges should be placed in the cups or hollows of the larger ones, and, if very small or delicate, in chip or card boxes, or a screw of stout paper. Sawdust or cotton should never be used. The box should be filled up and closely packed, but without crushing. In selecting from the rejected matter at high-tide mark, plenty of horny zoophytes should be put in, and especially those which are full of parasitical matters, as such have frequently growing on them the most minute and curious species of the sponge tribe, and also numerous minute and beautiful corals. If a large stone be appended to the sponge it is best to secure it in a corner of the box, by boring two or more gimlet holes near the angle; pass a string round the stone and through the holes, and drawing all tight from without, plug the holes and string firmly with wooden pegs, and cut off close to the box.

"The writer would also be *particularly* obliged by specimens of Spongilla, or fresh-water sponges, as he is engaged on a Monograph of that tribe. They are found in rivers, lakes or tanks, and pools, attached to dead wood, rocks or stones, and are occasionally found surrounding the branches of trees, dipping into the water during periodical floods; and if they contain their granular, seed-like bodies they are the more valuable. Dry them just as they come from the water. If it be deemed necessary to preserve parts or the whole of delicate specimens of either marine or fresh-water sponges in fluid, the best material is strong spirit, or water with a *considerable* excess of undissolved salt in it, but *never* alum. Jars or pickle and fruit bottles, well corked and sealed, or tied over with bladder, are the best vessels for the purpose."

The Editors of this Journal, or Prof. Gray of Cambridge, will gladly receive collections of Sponges and Spongillas made in this country, and forward them to Mr. Bowerbank. There are indications of two or more species of Spongilla in our lakes and streams, different from the two European species, and as yet undescribed; and the present opportunity for their thorough investigation should by all means be improved. A. G.

7. *Seeman's Botany of the Voyage of the Herald;* parts IX, and X.—The latter just issued, complete this creditable botanical work. It extends to 483 pages, and to 100 plates, all well chosen and well executed. The 9th fasciculus finishes the collections in North Western Mexico, and give a general introduction to the Flora of Hong Kong: the 10th comprises what purports to be a synopsis of the known plants of this island, 778 in number, a full index to the volume, and 14 pages reprinted to correct errors and give additional information. In one of them is corrected a mistake by which a *Tephrosia* was taken for so peculiar a plant as our *Galactia marginalis*, Benth. The Hong Kong *Compositæ* are elaborated by Dr. Steetz, with his usual conscientious care and good judgment; the *Orchidaceæ* by the younger Reichenbach; the *Cyperaceæ* and *Gramineæ* by Col. Munro; and the *Ferns* by Mr. John Smith. In a neat preface Dr. Seeman takes just credit to himself for having proposed only a very limited number of new genera and species, considering the extent of his collections and the number of little-known countries visited.* He attributes this in a good degree to his having had "the advantage of free access to the largest herbarium in the world, that which the liberality of Sir W. J. Hooker has thrown open to the scientific public; an advantage enabling me to identify most of my plants with already described ones, and preserving botanical literature from a series of synonyms with which under less favorable circumstances, it must and would have been hampered. * * Hence what at first would appear an unfavorable feature, will on second consideration prove perhaps one of the best recommendations of this work." We may add that this advantage would have been of small account, except for the untiring industry of the author and the great knowledge of those who helped him.

Dr. Seeman,—now personally known to the naturalists of the United States, which he has recently visited as the representative of the Linnæan Society to the Montreal meeting of the American Association,—is the editor of the *Bonplandia*, a Botanical Journal, now in the fifth year of its existence, published at Hanover, in monthly numbers, of small folio size. This, we learn is to be enlarged this year, and to contain some floricultural matter. In its new form we trust it will find additional subscribers in this country. The only drawback is the language, of which the German is the staple; but most of the technical matter relating to systematic botany is in Latin; and articles are admitted either in French or in English. A. G.

8. *Dr. J. D. Hooker: On the Structure and Affinities of Balanophoreæ* (separately issued from the *Transactions of the Linnæan Society of*

* To help on a little this laudable diminution of nominal species, we may remark that the only species which Dr. Seeman has proposed as new in the Flora of Western Esquimaux-land (and admirably figured,) viz.: his *Artemisia androsacea*. is doubtless *A. Senjavinensis*, of Besser, from the opposite coast. A. G.

London, vol. xxii). pp. 68, 4to, 16 plates.—Although read before the Linnæan Society nearly three years ago, this fine memoir was published only last summer. The delay has probably been owing. in great part, to the time requisite for the engraving of the very beautiful and elaborate plates which illustrate the memoir. It is a clear, patient, and philosophical elucidation of an extremely anomalous group of plants, and a succinct exposition of the principal lessons to be learned from their study, both organographically and systematically; and it bears the impress throughout of the spirit, freshness, and independence which so distinguish this author, and make all his writings so attractive and instructive. While the whole subject is developed in proper order, the divisions are not quite clearly marked out in the essay. The first sectional heading is "1. *Parasitism and Structure of the Rhizome.*" But there is no section 2 answering to the first, which, moreover continues, without a break, to treat of the general anatomy, organography, and morphology of these plants, the structure of the flowers, ovules, and seeds, and of the diverse doctrines which have been propounded respecting them. The *Affinities of Balanophoreæ* are then considered, under a special heading; their *Classification* is then the subject of a few general remarks; also their *Geographical Distribution and Variation.* Then a Synoptical Table of the genera is given; and the 14 genera with their known species (28 in all) are finally described and illustrated.

As to the structure and affinities of *Balanophoreæ*, and the curious questions that have arisen about their place in the natural system, Dr. Hooker, in the first place, affirms them to be truly phænogamous. It now seems strange that this should ever have been doubted. The arguments to the contrary, says our author, "all appear to have originated, on the one hand, in mistaking feeble analogies between the forms of organs that are not homologous, for affinities; and, on the other, in overlooking a multitude of positive characters. These arguments may be summed up as:—1. An erroneous view of the nature of the seeds, by Endlicher, Martius, Blume, and others, who describe them as a sporuliferous mass,—a term which, even if it were applicable, has no meaning. 2. An erroneous view of their origin being in a diseased state of the plants they grow upon, adopted by Junghuhn and Trattinick. 3. A supposed similarity in appearance to *Fungi*, and an erroneous idea that their appearance is meteoric and their growth rapid;—a theory advanced by Endlicher, who says of the horizontal rhizome of *Helosis* and *Langsdorffia*, "mycelio Fungorum quam maxime analogum." 4. The resemblance between the articulated filaments on the capitula of the *Helosideæ* and the paraphyses of *Musci;* and between the pistils of *Balanophoreæ* and the pistillidia of Mosses; strongly advocated by Griffith and Lindley. 5. The resemblance of the cellular and vascular tissues in some of their characters to some of those of Filices, as indicated by Unger and Gœppert. 6. A very peculiar view of the nature and relations of the parts of the female flower entertained by Weddell; who hence considers *Balanophoreæ* (together with *Rafflesiaceæ*) to approach nearer to Gymnosperms than to any other group of plants." Instead of discussing at length opinions which "had the authors who advocate them been sufficiently furnished with specimens and facts they would never have entertained,"

Dr. Hooker merely recalls attention to the essential facts that these plants exhibit true flowers with stamens and pistils, genuine ovules, and even embryo, and so accord in no one particular with Cryptogams. He shows moreover that the embryo is dicotyledonous in the few cases where it is sufficiently developed to manifest the character, and that the stem is constructed upon the exogenous plan. Even with these facts before him Lindley has retained his *Rhizogens*, as "logically a class;" as an intermediate form of organization between *Endogens* and *Thallogens*, and characterized by vegetation rather than fructification. But there is little or nothing really peculiar in their vegetation; and, as Lindley himself reduces the differences to questions of degree, it suffices to say that classes are not founded upon degradation of type, but upon change of type.

Viewing *Balanophoreæ*, then, as degraded members of the Dicotyledonous class, Dr. Hooker follows Brown and Griffith in regarding *Rafflesiaceæ* as near to *Aristolochiaceæ*, and in denying all affinity between these and *Balanophoreæ*. In searching for the affinities of the latter, Dr. Hooker is guided by the sound rule of disregarding "the negative characters, as those may be termed which are founded on the imperfection of organs;" and he takes the most perfectly developed species as the best exponents of the typical structure of any group,—a principle laid down, we believe, by Mr. Brown. This gives a substantial scientific basis for the estimation of affinity. Agreement *in plan of structure* is just what constitutes affinity; agreement in *grade of evolution* may indicate only distant analogy, can indicate only collateral relationship,—not to be neglected, indeed, but in itself of no account in assigning a family to its true position in the system. The principle as applied in the present case leads Dr. Hooker to the conclusion that the nearest relatives of *Balanophoreæ* are the *Haloragеæ*, a group itself "consisting for the most part of reduced forms of *Onagrariеæ*," or more strictly speaking, that the link which connects these plants with the higher forms of vegetation is furnished by *Gunnera*. The qualifying phrase above is appropriate; for it is hard to conceive of *Gunnera* with its minute embryo as a reduced *Onagraceæ*, while it is impossible to sever the chain of evidence which binds the genus to *Loudonia* and *Haloragis*. Be this as it may, Dr. Hooker has surely made a happy hit, in seizing upon *Gunnera* as the key to the true affinities of *Balanophoreæ*. Of all the objections that may be urged against this approximation not the strongest, but rather the least valid, in our opinion (so long as the question is one of alliance and not of co-ordination), is that to be derived from the habit and the imperfection of the foliar organs. Any type is liable to have its parasitic phase, and this is generally a degraded one in these respects; the Gesneriaceous has it in *Orobancheæ*, which it might with the greatest propriety include; the Scrophulariaceous graduates insensibly into similar parasitic forms; the Ericaceous has them in *Monotropeæ;* and the Cornaceous or Olacaceous degrades through *Santalaceæ* into *Loranthaceæ*.

It is quite probable that our author would deny the *degradation* in the latter case, judging from some points which he makes when considering whether the group of *Balanophoreæ*, "putting aside any consideration of its relationship with other orders, and regarding it *per se*, ... should abstractedly be considered as ranking high, or the contrary." This is an

abstraction of which we are hardly capable,—that of determining the rank of an order *per se*. Still our author's ideas are clear and clearly expressed: the comparison is really between these plants and the ideal plant-type. And what is wanting to make the comparison practical is a settled idea as to what constitutes the highest style of plant, and what is the relative importance of deviations from it;—questions too large to be entered upon here, if indeed the science is yet ready for their discussion, but which underlie the most important inquiries which good systematic botanists are everywhere tentatively prosecuting. "Assuming that the conventional definition of perfection in use among zoologists is applicable to the vegetable kingdom, and which argues that a high degree of specification of organs and morphological differentiation of them for the performance of the highest functions, indicate a high rank, Dr. Hooker ingeniously argues that "*Balanophoreæ* may in some respects be considered to hold a very high one;" and the points are presented under seven heads. Now we will not deny that the principles are logically applied in the present case, nor that considerations of the kind are perhaps as applicable to the vegetable as to the animal kingdom. But we should *a priori* expect that principles of fundamental importance in the latter could have no sound application to the former; that even such as relate to functions common to the two, or to structures analogous, would require to be based each upon its own ground. As to morphology, and as to what constitutes perfection of type, we should look to the fundamental differences rather than to the resemblance of the two for our starting point.

Plants, for obvious reasons, are constructed on the principle of extension of surface. Concentration or consolidation, wherever it occurs in the vegetable kingdom, is a special provision against some peculiar danger. Animals, on the contrary, are formed on the principle of restriction of surface. As if to withdraw them as much as practicable from the direct action of the external world, their shape is compact, their extent as individuals strictly limited, the external organs by which they take their sustenance comparatively few and small, while the most essential organs are safely sheltered within. Consolidation of organs and even their restriction in number, accordingly are not likely to be indications of high rank in the vegetable kingdom. Not the latter, because the object of the plant in vegetation is attained by the indefinite repetition of the same organs; nor the former, for the type of the plant is realized only in the distinct elimination of leaves from the axis. A Melon-Cactus, and a Cuscuta are low forms of plants as to vegetation. As it is a fundamental character of plants that their organs of reproduction are only specialized organs of vegetation; as the higher great divisions of plants are those in which the leaf-type is most apparent throughout; as the perfect accomplishment of the end in view,—the production, protection, and nourishment of the embryo even of the highest or most developed kind,—does not require the confluence of homogeneous parts, why should such confluence be regarded as indicating higher rank, merely because the type is more disguised in such cases? We see no sufficient ground for ranking a monopetalous plant higher than a polypetalous one on that account; and still less for regarding a *Loranthus* or a *Viscum* as the highest style of plant. On

the contrary, we incline to look upon the consolidation of heterogeneous parts in the blossom not as high specialization at all, but as want of development, i. e. imperfect elimination; and in this light those who maintain an inferior ovary to be one immersed in a receptacle, must needs regard it.

Again suppression or abortion of organs which belong to the type of the blossom cannot be considered as other than an imperfection, although the loss of the corolla is no great matter, and the abortion of one of the sexes little more. Still hermaphroditism is plainly in the type of the highest style of plant; while the opposite is the case in the animal kingdom. But we cannot here enter further into the discussion of this class of questions. No one feels more deeply than our author the want of fixed and philosophical principles for the subordination of characters and the study of affinities in plants; and no botanist of his age is more competent, or so well placed and furnished for the investigation of this problem, to which we invite him, as to a task worthy of his powers.

As to the rank of *Balanophoreæ*, if our author has demonstrated anything, it is that they belong to the highest class of plants, but that they are probably the most degraded members of it. A. G.

9. *Boussingault: Researches upon the influence which assimilable nitrogen in manures exerts upon the production of vegetable matter, and* (2.) *Upon the quantity of nitrates contained in the soil and in water of various kinds* (Ann. Sci. Naturelles, ser. 4, vol. 7, No. 1, 1857).—Several years ago Boussingault demonstrated, in the clearest way, that plants are incapable of assimilating the free nitrogen of the atmosphere. Two years ago, in a paper communicated to the French Academy of Sciences, he showed that nitrates eminently favor vegetation. He now shows, by decisive experiments,

(1.) That the amount even of ternary vegetable matter produced by a plant depends absolutely upon the supply of assimilable nitrogen (ammonia and nitrates). A plant, such as a sunflower, with a rather large seed, may grow in a soil of recently calcined brick, watered with pure water, so far as even to complete itself by a blossom; but it will only have trebled or quadrupled the amount of vegetable matter it had to begin with in the seed. In the experiments, the seeds weighing 0·107 grammes, in three months of vegetation formed plants which when dried weighed only 0·392 grammes,—a little more than trebling their weight. The carbon they had acquired from the decomposition of carbonic acid of the air was only 0·114 grammes; the nitrogen they had assimilated from the air in three months was only 0·0025 grammes.

(2.) Phosphate of lime, alkaline salts and earthy matters indispensable to the constitution of plants exert no appreciable action upon vegetation, except when accompanied by matters capable of furnishing assimilable nitrogen. Two plants of the same kind, grown under the same conditions as above, but with the perfectly sterile soil adequately supplied with phosphate of lime; alkali in the form of bicarbonate of potash, and silex from the ashes of grasses, resulted in only 0·498 grammes of dried vegetable matter, from seeds weighing 0·107 grammes; and had acquired only 0·0027 grammes of nitrogen beyond what was in the seeds.

(3.) But nitrate of potash furnishing assimilable nitrogen, associated with phosphate of lime and silicate of potash, forms a complete manure, and suffices for the full development of vegetation. Parallel experiments with nitrate in place of bicarbonate of potash, resulted in the vigorous growth of the sunflower plants, and the formation of 21·248 grams of organic matter, from seeds weighing as before only 0·107. This 21·111 grams of new vegetable matter, produced in three months of vegetation, contained 8·444 of carbon derived from the carbonic acid of the air, and 0·1666 grams of nitrogen. The 1·4 grams of nitrate of potash supplied to the soil contained, 0·1969 grams of nitrogen, leaving a balance of 0·0303, nearly all of which was found unappropriated in the soil.

Finally Boussingault made a neat series of comparative experiments, introducing into calcined sand the same amount of phosphate of lime and carbonate of potash, but different proportions of nitrate of soda, or in other words of assimilable nitrogen, and watering with water free from ammonia but containing a quarter of its volume of carbonic acid. The soil divided among four pots, each having two seeds of sunflower, (*H. argophyllus* was the species used in all the experiments); the pot

No.	1	received of	nitrate of soda,	0·00	grams.
"	2	"	"	0·02	"
"	3	"	"	0·04	"
"	4	"	"	0·16	"

The results of fifty days vegetation are given in the rate of growth, size and number of the leaves, weight of the product, &c.:

No.	1	made of	new vegetable matter	0·397	grams.
"	2	"		0·720	"
"	3	"		1·130	"
"	4	"		3·280	"

In No. 2 so little as three milligrams of assimilable nitrogen introduced into the soil enabled the plant to double the amount of organic matter. The proportion of the weight of the seeds to that of the plant formed was in

No.	1,	as	1 : 4·6 gr.
"	2,	"	1 : 7·6
"	3,	"	1 : 11·3
"	4,	"	1 : 30·8.

In no case did the nitrogen acquired by the plant exceed that of the nitrate added to the soil.

In the experiments where no nitrate was added to the soil, the two or three milligrams of nitrogen acquired by the plants during three months of vegetation, came in all probability from ammoniacal vapors and nitrates existing or formed in the atmosphere. To establish their presence, Boussingault arranged an apparatus which detected the production of some nitrates. And, in exposing to the air 500 grams of calcined sand, which had 10 grams of oxalic acid mixed with it, in a glass vessel with an open surface equal to that of one of the flower-pots used in the above experiments, the sand took 0·0013 grams of nitrogen from the air, of which a part was certainly ammonia.

The object of the researches of which a summary is given in the second paper was, to determine the quantity of nitrates contained, at a given moment, in one *hectare* of cultivated ground, one of meadow, one of the forest-soil, and in one *metre* of river or spring water. The quantity in the soil was of course found to vary extremely with the extremes of wet or dry weather. Garden soil, highly manured every autumn, contained on the 9th of August, 1856, after fourteen dry and warm days 316·5 grams of nitre in a cubic litre of soil. On the 29th of the month, after twenty rainy days, the same quantity of the same soil contained only 13 grams of nitre. The greater part had been dissolved out of the superficial soil.

Some specimens of forest-soil, in a state of nature, furnished no indication of nitrates: others gave 0·7 and 3·27 grams of nitre to the cubic metre.

The soil of meadows and pastures afforded from 1 to 11 grams of nitre to the cubic metre. Nineteen specimens of good cultivated land gave, four of them none; others from 0·8 to 1·33; the richer ones from 10·4 to 14·4, and one fallow, of exceptional richness, as much as 108 grams of nitre to the cubic metre. To the latter much calcareous matter had been added.

The soil of a conservatory, from which the nitrates would not be washed away by rains, contained 89, or 161, and some rather deep soil 185 grams of nitre in the cubic metre.

The sources of the nitre are not difficult to understand when we reflect that a manured soil, especially a calcareous one, is just in the condition of an artificial nitre-bed. The ultimate result of the decomposition of ordinary manure is a residuum of alkaline and earthy salts, phosphates, and nitrates, the latter, with the ammonia furnishing the assimilable nitrogen, all-essential to productive vegetation. In incorporating with the soil undecomposed manure, instead of the ultimate results of the decomposition, less loss is suffered from prolonged rains washing out the formed nitrates.

The soluble matters washed out of the soil are to be sought in the water. River and spring waters therefore act as manure by the silex and alkali, the organic matter, and the nitrates which they hold. The spring waters poorest in nitre of those examined contained from 0·03 to 0·14 milligrams of nitre to the *litre;* the richer ones from 11 to 14 grams in the cubic metre.

As to river-water; the Vesle in Champagne held 12 grams, the Seine at Paris 9 grams the cubic metre. These were the richest. The Seine at Paris carries on to the sea, in times of low water 58,000 kilograms, in times of high water 194,000 kilograms, of nitre every twenty-four hours. What enormous amounts of nitre must be carried into the sea by the Mississippi, the Amazon, and by every great continental river; and how active, beyond all ordinary conception, must the process of nitrification be over all the land; and how vast the supply of assimilable nitrogen for the use of the vegetation! A. G.

10. *Action of foreign Pollen upon the Fruit.*—In the last number of this Journal, p. 443, some facts were referred to which led to the supposition that pollen applied to the stigma may exert some specific action upon the ovary itself, independent of its action upon the ovules determining the

formation of the embryo. This was mentioned as furnishing the most probable clue to the explanation of the reputed fact that squashes are spoiled (that is the quality and appearance of the fruit altered) by pumpkins growing in their vicinity, and *vice versa;* and even that melons are spoiled by squashes; and this notwithstanding the fact, ascertained by Naudin, that distinct species of *Cucurbitaceæ* refuse to hybridize, although the various races of the same species cross with the greatest facility. It is generally agreed that the alteration of the character of the fruit is immediate, i. e., that it affects the ovary itself which has been contaminated by strange pollen. It might then equally affect the fruit whether the seeds were any of them fertilized or not; and in Naudin's experiments the application of pollen apparently caused the fruit to set, even when no ovules were fertilized.

Now a similar case of direct action of alien pollen upon the fruit, or grain, occurs in Indian Corn, and is familiar to every farmer in the country, in the form of grains of different varieties on the same ear. A decisive instance is before us in a small ear of Sweet Corn, grown in the vicinity of a patch of the common hard yellow variety; in consequence from three to six grains in every row have become yellow corn, while the rest retain the characteristic appearance of the sweet variety. It is not rare, where several sorts of maize are cultivated together, to find nearly all of them separately represented upon one ear. This must be the result, either of cross-fertilization of the previous year showing itself, not in a blending of the characters of the fruit of the progeny, but in a complete separation into the constituent sorts in the fruit resulting from one seed, which would be a wonderful anomaly, but no impossibility; or else, of an immediate action of the pollen the present year, as is reputed of squashes and melons. But the occurrence of three sorts of corn upon one ear goes far towards excluding the first supposition, since there can have been but two immediate parents to one embryo. A. G.

11. *Structure and development of the Flower and Fruit of the Pear;* by J. Decaisne. From a communication made to that active association, the Botanical Society of France,—of which an abstract is published in the Gardener's Chronicle of Nov. 14th, we learn that Decaisne has proved by direct observation of the development, the correctness of that view respecting the structure of the pomaceous fruit which we have always maintained on general morphological grounds. The pips are the true pistils: they are separate and free at their first appearance: a little later a growth from the receptacle forms an open cup around them, ends by completely investing them, and becomes the flesh of the core. In the Pear, as the base of the at first sessile flower-bud elongates into a peduncle, the upper part of this thickens with the bud itself, and forms the tapering lower part of the pear, which therefore, below the carpels, is formed of the stalk, as absolutely as in *Anacardium* or *Hovenia*. From these observations and others upon *Melastomaceæ*, &c., Decaisne concludes that the orthodox view of the structure of the flower, "as explained by our illustrious masters, R. Brown, DeCandolle, and Jussieu" is demonstrably correct; that "it is not necessary to call into account that axis which is at the present day so often and so willingly appealed to for explaining the structure of flowers and fruits;" that "it is not impossible to

bring under the common law of organization the ovaries with a free central placenta, whose differences from ordinary ovaries are more apparent than real;" that most probably placentation always, in spite of appearances, belongs to the ovarian leaves. We are pleased to find that the experience of this eminent botanist has brought him into agreement, as regards the conception of species, with the views of those whom we must regard as the soundest workers and writers of the present day, and those on whom the hopes of the science rest. He states that if he had the *Plantagineæ* to elaborate anew, he should not hesitate to reduce considerably the number of species, "and perhaps to refer some entire sections to a single specific type." Perhaps even the greater part of two sections, we may add; for of two sections in the Prodromus, one is founded upon substerile and the other upon truly fertile forms of the same species, or set of species: and in another part of the genus one wide-spread American species figures under at least a dozen names. See notes on *Reports of Pacific Railroad Explorations*, vol. 4, p. 117.* A. G.

12. *Naturhistoriske Bidrag til en Beskrivelle af Grönland;* af J. Reinhardt, J. C. Schiödte, O. A. L. Mörch, C. F. Lütken, J. Lange, H. Rink. 8vo, pamph., pp. 172, with map. Copenhagen, 1857.—This work contains a complete catalogue of the fauna and flora of Greenland as far as known, together with some account of the geology and meteorology. It is a work of great importance to North American naturalists, relating as it does to our northern Fauna and Flora, and containing many valuable remarks upon the genera and species, particularly in regard to their synonymy. The zoological portion only will be noticed here. The Mammalia, of which there are 27 species, Birds, 111 species, Fishes, 69 species, Crustacea, 138 species, and Annelida, 87 species,—are elaborated by Reinhardt; the Insects by Schiödte; the Mollusca, 211 species, and Acalephæ, 33 species, by Mörch; the Echinodermata, 29 species, and Polypi, 7 species, by Lütken.

In the catalogue of testaceous molluscs, our conchologists will not be surprised to see many of our familiar species appearing under strange names. In this department of zoology, the names, particularly those of the genera, seem to undergo a periodical change. Of Pulmonates there are eleven species, seven land and four freshwater. *Vitrina angelicæ* hardly differs from *V. pellucida;* if distinct it is probably identical with *V. limpida*, Gould. The various divisions of *Helix*, as *Conulus*, *Helicella* and *Helicogena*, are adopted as genera.

Our *Bulla triticea* is regarded as the same as the European *Cylichna alba*. *B. Reinhardi*, Möll., is catalogued as "*Cyl. insculpta*, Totten," which however is only a synonym of *Bulla solitaria*, Say. *Dendronotus Reynoldsii*, Couth., is considered a distinct species from the European *arborescens*, to which it is generally referred by our naturalists. Four species of *Velutina* are mentioned, *V. flexilis*, *lanigera*, *haliotoides* and *zonata*. The classification adopted is singular in some respects; for example it is somewhat startling to see *Littorina* introduced between *Velutina* and *Natica;* while *Rissoa* and its allies are placed between *Natica* and *Cerithium*. *Natica* is divided into four genera, *Natica*, *Lunatia*, Gray,

* These volumes some of them contain much interesting botanical matter. We may call attention to them when the remaining volumes are published.

Mamma, Klein, and *Amauropsis*, nov. gen. Our *Natica clausa* is *N. affinis* (*Nerita*) Gmel. *N. grönlandica*, Beck., is a *Lunatia*. *N. immaculata*, Totten, is said to be the young of *N.* (*Mamma*) *borealis*, Gray, 1839; if so, Totten's name has priority. *Natica helicoides*, Johnst., is called *Amauropsis islandica* (*Nerita*), Gmel. *Adeorbis costulata* is placed in *Cyclostrema*, Marryat. *Mangelia turricula* of our coast is regarded as distinct from the European *Fusus turriculus* and styled *Defrancia scalaris*, Möll. The name *Tritonium* is adopted for *Buccinum*, including *B. glaciale*, *Donovani*, *undatum*, *grönlandicum*, *Hancockii*, *tenue*, *undulatum*, *Humphreysianum*, and *ciliatum*; while the name *Fusus* is retained for *F. despectus*, *tornatus*, *islandicus*, *norvegicus*, etc. The rules of nomenclature would however seem to require that *Buccinum* should be retained, being the older name, whether *Tritonium* be preferred to *Fusus* or not. *Fusus clathratus* is placed under *Murex*. *Spirialis Gouldii*, Stimpson, is correctly referred to *Limacina balea*; but we cannot see why the name *Heterofusus* of Fleming, founded on an error, should be preferred to the more euphonic one of Souleyet. *Margarita undulata* is *M. grönlandica*, Chemn., 1781. The names *M. cinerea* and *argentata* of our naturalists are acknowledged over European synonyms.

Patella candida is regarded as identical with *P. cæca* and arranged in *Lapeta*, Gray (*Cryptobranchia*, Midd.). *Cemoria*, Leach, is retained over *Diadora*, Gray. Only two Chitons, *C. marmoreus* and *C. albus*, are catalogued, and the subgenera of Gray are adopted. Eight species of Cephalopoda are found in the list, which seems a large number in view of the fact that only three are known to occur on the eastern coast of the United States. The Greenland *Teredo* is set down as "*T. denticulata*, Gray, 1850," with *T. dilatata*, Stimpson, as synonym; an error of date, the latter name having been published, with description, in Oct. 1851; Gray's name in the Ann. and Mag. Nat. Hist. for Nov. 1851, with no description. *Cyrtodaria* of Daudin is retained for *Glycymeris*, Lamk. *Tellina proxima* is *T.* (*Macoma*) *sabulosa*, Spengler, according to Mörch. We are not aware however of its having been called *Psammobia sordida* either by Couthouy or Gould. *Tellina fusca* (*grönlandica*, Beck) occurs under the name of *T. tenera*, Leach, in Rozet's Journal, 1818. Should this name hold, our common species *T. tenera*, Say, 1821, must receive a new designation, and might be called *T. agilis* in view of its quick and sprightly movements. *Serripes*, Beck, is preferred to *Aphrodita*, Lea; which latter name however has priority, and should stand unless rejected on account of its occurrence in other departments of the animal kingdom. *Nuculana*, Link, is retained over *Leda*, Schum.

Several of our New England mollusks, whose limits were not before known to extend so far north, are mentioned in the catalogue: as *Bulla insculpta*, *Eolis Bostoniensis*, *E. salmonacea*, *Rissoa eburnea*, *Thracia truncata*, *Montacuta elevata* and *Kellia planulata*. Many other points of interest to our malacologists may be found in the catalogue, but our space will not permit of further notice of them here.

We will add a few brief remarks upon Lütken's catalogue of the Echinodermata. Of this class we find 9 Holothuridæ, 1 Echinus, 8 Asteriadæ, and 11 Ophiuridæ. *Cucumaria* is retained instead of *Pentacta*, Goldfuss, notwithstanding the latter has priority. *C. Koreni*, Ltk., nov. sp., is

Pentacta calcigera, St., Bost. Proc., 1851. *Cuvieria* is united to *Psolus* and with good reason. The new genus *Eupyrgus* seems related to *Psolus*; *Myriotrochus* of Steenstrup to *Chirodota*. *Asteracanthion grönlandicus*, Stp., scarcely differs from *A. littoralis*, Stimpson, Synopsis Inv. of Grand Manan, p. 14, while *A. problema*, nov. sp., is identical with *A. albulus*, St., loc. cit. *Ophiura Sarsii*, Ltk., n. s., is common on our coast and here considered a variety of *O. ciliata*. *O. squamosa*, Ltk., nov. sp., is common at Grand Manan, and is *O. robusta* of the Synopsis. *O. robusta* of Ayres seems to include both *O. squamosa* and *nodosa* of Lütken. It is proper to state that the above identifications are established upon actual comparisons of specimens. W. S.

13. *Contributions to The Natural History of the United States of America*; by Louis Agassiz. First Monograph in three parts: I. Essay on Classification; II. North American Testudinata; III. Embryology of the Turtle. In two volumes 4to of 640 pages, with thirty-four plates. 1857. Boston: Little, Brown & Company. London: Trübner & Co. Subscription price, per volume, $12.

These two quarto volumes on American Zoology are the first of a series of ten volumes, which Prof. Agassiz has in course of preparation. It is most honorable to the country and a high tribute to the author, that this great work is appearing under so liberal auspices.

Eleven years ago Prof. Agassiz landed in America: since then his labors have been incessant, and as a consequence, a large amount of drawings and manuscripts connected with American zoology had accumulated on his hands. The seashores here opened to him a field in zoology he had not hitherto enjoyed, the rivers and lakes were full of life that had new revelations for him, the whole land in every direction tempted a mind in love with all forms of nature, and nearly every department of zoology had therefore been the subject of special researches. Encouraged and aided by a distinguished friend, Mr. Francis C. Gray of Boston—since deceased—the plan of publication by subscription was set on foot. Prof. Agassiz alluding to his benefactor and the subscription, says, in his Preface:

"He took the whole direction himself, awakening attention to it by personal application to his friends and acquaintances, by his own liberal subscription, by letters, by articles in the journals, and by every means which the warmest friendship and the most genuine interest in science could suggest. He was rewarded beyond his utmost hope or mine, by the generous response of the public to whom he appealed. We had fixed upon five hundred subscribers as the number necessary, to enter upon the publication with safety; and we had hoped that the list might perhaps be increased to seven or eight hundred. At this moment it stands at twenty-five hundred: a support such as was never before offered to any scientific man for purely scientific ends, without any reference to government objects or direct practical aims,—although I believe no scientific investigations, however abstruse, are without practical results. My generous friend did not live to witness the completion of the first volume of the series, which without his assistance could not have appeared, but he followed with the deepest interest every step in its progress, to the day of his death;—he did live, however, to hear the echo which answered his appeal to the nation, in whose love of culture and liberality towards all

intellectual objects he had felt so much confidence. From all the principal cities, and from towns and villages in the West, which a few years since did not exist; from California, from every corner of the United States,—came not only names, but proffers of assistance in the way of collections, and information respecting the distribution and habits of animals, which have been of the utmost assistance in the progress of the work."

Prof. Agassiz, from his first arrival, has identified himself with American science. He left Europe behind him, and cared not even to seek European channels for his publications, although they have the advantage of wider circulation and repute. He has often spoken with strong disapprobation of that petty ambition sometimes seen on this side of the waters, to contribute to a foreign periodical, rather than those of the land. His lot and mission were here; and the response he has met with from the country is testimony, not simply to his science, but to the noble feelings of the man. On certain of his views men may differ; but as to honest purpose in research, thoroughness of investigation, breadth of philosophical ideas, and beauty of actual results, there can be but one opinion.

The work as projected, was to consist of ten volumes of about 300 quarto pages and twenty plates each. The subject intended for the first volume expanded under the hands of the author through the extension of his researches, and also still more from the introduction of a series of chapters on the philosophy of classification and a kindred topic of general interest growing out of it—Creation the work not of physical agencies, but of a personal Creator. The text was thus enlarged to the size of two volumes and the plates also were increased in number to nearly double that projected. The publishers have therefore issued the whole in two volumes. Prof. Agassiz states in his Preface that in the third volume, which is already far advanced, the deficiency in the number of plates will be fully made up, the subject requiring very numerous illustrations.

Prof. Agassiz, besides his many acknowledgements of aid received in the way of specimens and information, mentions Mr. James E. Mills and Mr. H. James Clark as valuable assistants in his scientific researches, the latter especially in microscopic dissections and drawings. He gives high commendation to Mr. A. Sonrel, the artist, whose labors as draftsman have contributed to his works now for twenty years. The beauty and finish of the plates fully justify the remark respecting Mr. Sonrel in the preface—"The mastery he has attained in this department, and the elegance and accuracy of his lithographic representations, are unsurpassed, if they are anywhere equalled."

As we shall return to these volumes again, we here give only a list of the topics of which they treat.

Vol. I, Part I. *Essay on Classification.*

Chapter I. The fundamental relations of animals to one another and to the world in which they live, as the basis of the natural system of animals: under which head, the author treats of—the actual foundation in nature of the true zoological system or classification,—the unity of plan throughout the diversified types—the distribution of the same types over widely diverse geographical regions, and as widely diverse geological ages,—the permanency of types and the immutability of species,—the relations between plants and animals and the surrounding world,—em-

bryology a basis for determining the rank of species—succession in geological time a basis for deciding approximately upon rank;—all of which topics, besides others not here enumerated, are so handled as to bear directly on the question of creation by physical agencies, giving it a decided negative reply.

Chapter II. Leading groups of the existing systems of animals—a philosophical disquisition on the true significance of the grades of subdivisions in the kingdoms of life, the nature of species, genera, families, orders and classes.

Chapter III. Notice of the principal systems of zoology, including observations on the systems of Aristotle and Linnæus; the *anatomical* systems of Cuvier, Lamarck, Ehrenberg, Burmeister, Owen, von Siebold and others; the *physio-philosophical* systems of Oken and McLeay; and the *embryological* systems of Döllinger, von Baer, Vogt, etc.

Part II. *North American Testudinata.*

Chapter I. The Order of Testudinata, its rank, classification, general characters, anatomical structure, geographical distribution, geological history, etc.

Chapter II. The Families of Testudinata.

Chapter III. North American genera and species of Testudinata—their characters, distribution, etc., for the several families.

Part III. *Embryology of the Turtle.*

Chapter I. Development of the egg from its first appearance to the formation of the embryo.

Chapter II. Development of the embryo from the time the egg leaves the ovary to that of the hatching of the young, including the laying of the eggs,—the deposition of the albumen and formation of the shell,—the absorption of albumen into the yolk sac,—the transformations of the yolk in the fecundated egg,—segmentation of the yolk,—the whole egg is the embryo,—foldings of the embryonic disc and successive stages of growth of the Turtle,—formation and development of the organs,—histology,—chronology of the development of the embryo.

The young of various species and the several successive phases in embryological development are illustrated with details in the plates, all of which are crowded full of figures.

In another number, we propose to give an abstract of some of the views brought forward in these volumes.

IV. ASTRONOMY.

1. *New Asteroids.*—The number of small planets already discovered between Mars and Jupiter is *fifty*.

The 46th was discovered by Mr. Norman Pogson, Oxford, Eng., Aug. 14, 1857;—the 47th (of 11th magnitude,) by Dr. R. Luther, Bilk, Sept. 15, 1857;—the 48th (11th magn.) and 49th (10th magn.) by Mr. H. Goldschmidt, Paris, Sept. 19 and 22, 1857;—the 50th (Virginia,) of 11th magnitude, by Mr. James Ferguson, Washington, D. C., Oct. 4, 1857.

2. *New Comets.*—The Fifth comet of 1857 was discovered by Mr. Klinkerfues, Göttingen, Aug. 20, 1857. In September this comet became visible to the naked eye, and had a tail about 3° in length.

The Sixth comet of 1857 was discovered by Mr. Robert Van Arsdale, Newark, N. J., Nov. 10, 1857. It appeared like a faint nebula.

3. *New Double Stars discovered by Mr. Alvan Clark, Boston, U. S.; with appended Remarks*, by the Rev. W. R. Dawes. (From the Proceedings of the Royal Astronomical Society of London, Vol. xvii, No. 9).

No.	Designation.	R. A.	N. P. D.	Mag.	Approx. Dist.	Tel. of Discovery.	Date of Discovery.
		h m	° ′		″	Inch.	
1	∗ Andromedæ	0 13·5	57 49	7, 7	0·4	7½	Oct. 1856.
2	95 Ceti	3 10·7	91 29	5½, 10	0·8	7½	20 Dec. 1853.
3	Weisse's Bessel, vi, 109	6 4·3	94 38	6½, 9	1·1	7½	6 Feb. 1854.
4	Wiesse's Bessel, vi, 1291	6 42·2	104 59	6, 9	1·0	4½	17 Feb. 1852.
5	8 Sextantis	9 45·1	97 24	6, 6½	0·6	4¾	7 April, 1852.
6	∗ Corvi	12 0·3	109 32	6		4¾	19 May, 1852.
7	μ Herculis, B. & C.	17 40·6	62 11	10½, 11	1·8	7½	July, 1856.
8	∗ Herculis	17 47·4	60 17	8½	0·3	7½	July, 1856.
9	∗ Herculis	17 48·7	60 9	10, 10½	1·1	7½	July, 1856.
10	21 Sagittarii	18 16·4	110 37	5, 8	2·5		
11	Weisse's Bessel, xviii, 391	18 17·2	91 39	7, 7½	0·5	7½	30 July, 1854.
12	Weisse's Bessel, xix, 1273	19 50·6	92 38	7½, 8	0·9	7½	18 July, 1854.

"The places of these difficult objects have been from time to time communicated to me in letters from their discoverer, who has adopted the plan of testing the efficiency of his object-glasses when completed by sweeping for *new* double stars of the last degree of difficulty, rather than by the examination of objects whose character was previously known. At first, under the impression that every such object in the northern hemisphere visible with telescopes of moderate aperture must already have been picked up and registered during the careful examinations of that portion of the heavens by the Struves with the Dorpat refractor of 9·6 inches aperture, and with the Poulkova of 15, Mr. Clark confined his search to the southern hemisphere; and his diligence and skill were rewarded by the discovery of several interesting objects, which, it might be supposed, would hardly have escaped the Dorpat telescope if they had been as decidedly double in 1826 as they are now. Latterly, however, having ventured to extend his researches northward, he has made some discoveries which are almost startling (especially the duplicity of the very minute companion of *μ Herculis*), and are sufficient to show that there is much which may be achieved by a diligent use of instruments of moderate dimensions, provided they are also of extreme perfection. A few notes on some of the most interesting of these objects may not, perhaps, be unacceptable.

"1. This star has a good altitude at Poulkova, and its magnitude entitles it to a place in Otto Struve's catalogue. Its omission there may reasonably lead to a suspicion that it has a binary character, and has of late years come out from a state of apparent singularity. I have obtained an approximate measure of this star, but have not examined it under sufficiently favorable circumstances.

"2. Of 95 *Ceti* I have repeatedly obtained measures; but it is a delicate object; and from its moderate altitude here requires fine circumstances. The small companion, which has a purplish tint, is faint and close, and may, therefore, have easily escaped detection at Dorpat.

"5. Notwithstanding the moderate meridional altitude of 8 *Sextantis* at Dorpat (about 24°), it may reasonably be doubted whether its duplicity

would have been left to be discovered with a 4¾-inch object-glass, however perfect, if no change had occurred in its appearance since Struve's scrutiny of that part of the heavens.

"7. The position and distance of the small star with respect to *μ Herculis* were observed by Struve at Dorpat on one night in 1829, on two nights in 1832, and on three in 1836; and also on one night at Poulkova with the 15-inch refractor in 1851. Yet no suspicion was recorded on any occasion of the companion being double. It is, therefore truly astonishing that Mr. Alvan Clark should have detected its unsuspected duplicity with an object-glass whose aperture is only 7¾-inches! I have succeeded in measuring it pretty well in position, but only approximately in distance; the faintness of the components almost forbidding the slightest illumination, though they bear a high power on Clark's 8-inch object-glass,—about 700 suiting them best. My results are P=58°·97; D=1″·85±. It has been shown by Struve (*Pos. Mediæ*, p. ccxvii.) that *μ* and its companion have a common proper motion; so that in this respect they are similar to *μ* and *μ*² *Boötis;* and as the companion of *μ Herculis* has now been discovered to be *double*, it only remains that it should prove to be *binary* to render the resemblance complete. It is, therefore, earnestly commended to the attention of those observers who possess telescopes competent to deal with it. As the small star *precedes* the large one, the former is properly *μ*¹, and the latter, *μ*², if that nomenclature be adopted.

"8. This star is about as difficult as the closest of the Poulkova catalogue; and though on a fine night elongated with the 8-inch object-glass I now have in use, would require the full power of a 15-inch refractor fairly to divide it. That it attracted Mr. Clark's attention as a double star is sufficient to prove that his eye as well as his telescope must possess extraordinary power of definition.

"9. This double star forms a good introduction to the small one of *μ Herculis;* its components being brighter by about half a magnitude of my scale. With my 8-inch object-glass and power 697, I have obtained, P=229°·48; D=1″·119.

"11. A very difficult object, though decidedly elongated with a 7½-inch aperture. My measures in 1854 gave, P=178°·10; D=0″·425, the latter a mean of two estimations.

"12. A neat and not very difficult object; it ought certainly to have been seen at Dorpat if it were as separate then as it is now. My measurements with Clark's 7½-inch object-glass gave in 1854, P=333°·76; D=0″·863."

"Haddenham, Thame, July 9, 1857."

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Prof. Richard Owen of Nashville, Tennessee, on the Outlines of the Continents.*—In noticing in our last number the communication of Prof. Peirce on the tangency of the great circles in which the coast lines lie with the Arctic and Antarctic circles, we stated the fact that the same view had been brought out in a work published by Prof. Owen, but by mistake made the publication of Prof. Owen's work subsequent in date to the first announcement of Prof. Peirce's views. A communication from Prof. Owen makes it clear that his publication has the priority, by

two or three months. His account is explicit. As some general interest is attached to these views, whatever their bearing or geological importance, we cite a few paragraphs from Prof. Owen's work. Aiming to bring out the "fixed rules" in the earth's structure he says; p. 36—

"The first step, in accordance with the above plan, is to collect the facts regarding the *Direction of the Coasts*, in their great outlines; also of the mountain ranges, rivers, etc.

"Here we at once perceive, if we elevate the north pole of the globe 23½° above the horizon, that, when we revolve the globe, we bring many of the great continental coasts, as well as shorter gulf and island coasts, successively to the horizon, proving their parallelism: for instance, the *west* of North America, (California,) part of the western coast of Southern Africa, (coast of Guinea), west coast of Arabia, India, Birmah, Malaya, Sumatra, Corea, Australia, and Borneo.

"Now depress the north pole 23½° below the horizon, and you bring the *eastern* coast of North America, (Atlantic seaboard of the United States,) the *east* coast of Southern Africa, and the general trend of the eastern Asiatic coast, (between Behring's Straits and the Gulf of Siam,) successively parallel to the horizon.

"A third set of coasts will be found parallel to the horizon, when we elevate the *equator* 23½°; in which case the west coast of Central America, the Northern coast of South America, the general direction of the Mediterranean coasts running from east to west, will be included in the number, and will be observed to be chiefly intertropical; while those previously pointed out are, for the most part, between the equator and the arctic circle or the equator and antarctic circle."

The origin of this system of lines is attributed to "solar influence in some of its modifications."

Prof. Owen also brings in 23½° as a distance, making what appears to us a fanciful or unmeaning use of it. He observes, p. 44,

"Not to weary the reader with similar details, which he can multiply indefinitely at his pleasure, it will suffice to enumerate a few prominent distances spanned by 23½°, and several smaller measurements which seem to be the eighth part of 66½°, or nearly 8¼°; near enough at least for practical purposes.

"The former of the two measurements, viz., 23½°, will be found to be the distance from the Alps to Palestine, as well as, in a due south direction, to the Tropic of Cancer; also from the Alps successively to the Madeira Islands, to Mount Hecla in Iceland, to near the Malström, to Lake Onega, and to the eastern shore of the Black Sea.

"It also measures from Palestine to the Baltic, to the Sea of Aral, to the south coast of Arabia, and to the Gulf of Genoa, and indicates the distance from Africa to the nearest point in South America.

"The smaller measure (about 8¼°) will be found to reach from the north of Scotland to the British Channel; from the west of Ireland to Flanders; from the west of Scotland to Denmark; from the Malström to the Baltic; from the Baltic to the Adriatic; from Calais to Genoa; from Brest to Marseilles; from Cape Finisterre to Cape St. Martin in Spain; from Genoa to Etna; from Tunis (ancient Carthage) to Barca; and from the Alps to the Gulf of Tarento," etc.

As we have expressed our dissent from the general principles of the work, we cite further a few paragraphs in order to give more definiteness to our objection, and at the same time to show those who may be interested, the nature of some of the discussions in the volume.

The subjects of the chapters of the work are—1. Physical, Statical or Geographical Geology; 2. Dynamical; 3. Anatomical; 4. Botanical; 5. Zoological; 6. Anthropological and Ethnological; 7. Pathological and Therapeutical; and last (8.) Ethical Geology.

Almost immediately following the last citation, in the 2nd chapter, he states, and afterwards discusses this announcement, p. 45:

"*The different successive geological periods will be found more recent, and less dense in structure, as they leave the north pole and approach the equator.* Although certain layers probably invest the globe, in a succession never inverted, yet, where upheaved, the edges or vertical sections of these formations appear to have been brought to the surface along concentric (or subconcentric) lines, which are parts of great circles, intersecting each other in such a manner as to form equilateral spherical triangles on the earth's surface: each angle of intersection being equidistant from our present north pole; also in such a manner as to cause hypozoic outcroppings in the smaller triangles, palæozoic in the next, and cainozoic in the larger."

On the next page occurs the following strange paragraph:

"By carefully laying down, on accurate maps, all the prominent points at which the Hypogene rocks are found in close proximity to Secondary rocks, and the latter again to Tertiary rocks, rejecting a few anomalies which occur chiefly at or near the above described longitudinal lines of upheaval; by carefully noting the chief localities in which coal and the ordinary metals have been found, there seems no doubt that these geological lines of junction and of greatest metalliferous surface-wealth, form as already stated, equilateral spherical triangles, the three sides of each of which are formed by the intersection of great circles, and the apex of which is to be found very nearly if not quite at the terrestrial North Pole; it is consequently probably the apex of a nucleiform spherical tetrahedron, on the curved faces of which there appear to have accumulated successive layers of deposition. However, whatever the theory may be, the practical result is, that by following the lines indicated on the map we connect nearly all the points at which mineral wealth has thus far been found, and in which ranges therefore we may most reasonably expect again to find it, at intermediate points or on extensions of those lines."

Chapter III. is devoted to "Anatomical and Physiological or Stratigraphical Geology—an attempt to demonstrate the analogy between organic structures and geological strata;" and after discussing the general nature of organic structures, the author draws out the analogy in the following geological extravaganza (pp. 84–88):—

"Our planet, perhaps, typifies an ovule from the solar matrix: in its earlier igneous, chaotic state, it bore analogy to the yet undeveloped amorphous structure of vegetable ovules and the animal ovum. Like them it had at an early period a nucleus, on which, after a time, air and moisture deposited additional materials, derived from the matrix. At a yet later period, a part of these same materials were carried in mechani-

cal mixture, partly in chemical solution, to promote the development of later formations, forming new continents, etc.; just as a portion of the seed (the albumen) and the food-yolk of the egg go to nourish the expanding germ.

"The separation of continents typifies the propagation by off-shoots, or artificially by cuttings, in plants; and seems to resemble the fissiparous mode of reproduction observed among the lowest animals. In some of the earlier cataclysms, we have the type of the ruptured Graafian vesicles, while at a final convulsive deluge, the period when the Western Continent and Australia were detached, and when possibly the moon, as a terrestrial ovule, was thrown into space, we readily recognize the type of ruptured pericarpal dissemination of seed, in the vegetable world, of completed incubation and parturition in the animal kingdom.

"But the analogy may be carried much farther: the earth, like man, has its mountain masses giving stability to the length and sometimes the breadth of the land, just as the skeleton forms the framework of attachment for muscles, etc. The materials solidified at the earliest periods are crystalline, depositing materials around central points, as the earliest bones, those of the head, commence their ossification by the arrangement of cells around a centre, or as the earliest animals partake of the radial type. At a later period the same materials (the serous layer of the germ-yolk in the animal, the detritus of igneous materials in the mineral world) furnish an ample deposit, which, in the long bones of the limbs, (femur and humerus,) in the muscles, etc., now partake of a lamellar appearance, the result of cells arranged in layers, viewed in their vertical sections. On the earth's crust, these deposits are carried by aqueous action from the high and hard mountains, and are afterwards consolidated, by heat, pressure, and electrical forces, into the sedimentary rocks of the various later periods.

"When an abundant supply of carbon has been furnished for the growth and subsequent decay of vegetation on the earth's surface, we then have the type of an extra-uterine nourishment.

"We have the coal period forming its vast layers of carbonaceous deposits, which, by slow chemical action under a portion of the earth's crust, the evolution of various gases, and the formation of new compounds, aid in keeping up the temperature of the internal earth. The water, trickling through the earth's pores, dissolving and carrying many saline and other ingredients to the ocean, is the type of the early lacteal products in the animal mingling with the venous blood, to be carried to the great centre of circulation, the heart, as the small streams unite into large, and carry the dissolved materials to the great deltas, and finally into the ocean gulfs.

"Turning our attention first to the Western Continent, we find, as just stated, the smaller streams anastomosing, (as the veins do to form the vena cava,) and at last discharging the chief waters of North America, by the Mississippi, into the Gulf of Mexico; while the Orinoco, Amazon, and Rio de la Plata send the inosculated waters of South America also toward the same gulf, through the currents tending to the Caribbean Sea.

"Here we have the type of that venous or vitiated blood, which is now thrown into the great central heart, and thence propelled, in the Gulf-

Stream, chiefly north and east, toward those regions where the ocean is less salt, as the Baltic; entering also the Mediterranean, and leaving there large saline deposits, the water of the ocean is evaporated by the heat of the sun, increased in intensity by reflection from Africa's sandy Sahara; and, thus purified, the aqueous vapor mingles with the atmosphere, to be carried along by its currents, until the accumulated humid contents of a cloud (typifying arterial circulation) descend, when its temperature has been depressed to the dew-point, as a pure deposition of aerated water, upon the thirsty earth, and filter through the loose soil, to carry nourishment to that earth, its plants and animals, and again perform the same circulating course of evaporation, purification, and condensation.

"The atmosphere, then, besides forming the type of aerial communication between parent and offspring, as indicated in the tabular view, is the type too of the great aerating organ, the lung, (whether under the form of external branchial tufts or internal parenchymatous structure, forming pulmonic sacs.)

"If the above be true, I would ask the scientific man not to sneer, when I hesitatingly inquire, whether it is possible that in the Western Continent we have the male type, of greater length with less breadth, even the type of the air-breathing animal, with its vast central air-caverns; and whether, at a former period, it foreshadowed the usual fœtal curvature *in utero*, before extension; whether in the Eastern Continent we have the maternal type, the cretaceous period corresponding to that of lactation, the greatest pelvic width typified in the highest Himalayas, and the water-breathing type with its central intercommunicating whirlpools?

"Well aware of the ridicule to which I expose myself, and feeling keenly the criticism of those competent to decide, I yet am impelled by a sense of duty to ask these questions, not as mere matters of speculative interest, but as queries, the answers to which may lead to important practical hygienic results, such as it will be attempted more especially to develop in some of the remaining chapters.

"Should the *probability* of any of the above analogies be admitted, Australia at once establishes its claim to the placental type, as well as on account of its former position, and the great evidences that the upper layers have been torn away, leaving an arid country around the Dead Sea some thirteen hundred feet below the level of the Mediterranean; as also by the marked peculiarities of its flora, (for instance, its leafless acacias, the petioles of which retain the nourishment that they should transmit to the leaves,) and of its noted fauna, among which so many belong to the order Marsupialia, exhibiting a tendency to an extra-uterine pouch or enormous development of the nipple integument, while other animals of that anomalous continent, and its detached New Zealand, form the link between the oviparous bird and viviparous mammalia, the Monotremata.

"In the attraction exerted by the moon over the tides, we have the type of the periodicity, to be enlarged upon more in the Chapter on Pathological variations; and it may suffice now to ask again, whether we have not, in the periodical flux and reflux, the type of normal and ab-

normal, regularly recurring exacerbations, as in intermittent and remittent fevers, in the periodical excretions, alvine, urinary, etc., in the catamenia, and even in the arrival and departure of epidemic agency?

"To recapitulate, in a more connected form, this comparison of inorganic with organic phenomena, we observe that the older rocks (the Hypogene, crystalline, non-fossiliferous) are chiefly found in the arctic and antarctic regions, (although, also, more or less accompanying every period;) next to those come the Secondary, (Palæozoic and Mesozoic rocks,) chiefly in the north temperate zone; while nearer the tropics are chiefly developed the Tertiary and newer formations. The Hypogene rocks, being of all ages and forming the hardest and highest mountain ranges, as well as in other particulars to be pointed out below, resemble the serous layer, which forms the great framework and consolidates at different periods of life. The Secondary formation resembles the vascular layer; and the Tertiary is the analogue of the mucous layer, although in the animal it is more frequently an internal than an external layer."

Remarks on these passages are unnecessary.

2. *On the Supposed Meteorite from Marblehead;* by A. A. Hayes.—A part of the mass of this substance, which was first described in a Salem paper a few days since, having been sent to me for chemical analysis, it has proved to be *an artificial product of the arts.*

The piece examined was externally black; its surface was channeled owing to numerous semicylindrical elevations, produced by the flow of jets of fluid mineral matter. Internally the color was blackish grey, the compact parts being crystalline; while numerous elongated and spherical bubble cavities rendered other parts porous and uneven. Much of the recent as well as the exposed part of the fracture exhibited a pseudo-metallic lustre of the color of copper, which also appeared in the cavities. A part of the exposed surface was coated by a light yellow ochry covering, easily removed from the black glazed surface below. When fractured, the glazed surfaces of the small streams of once-fluid matter were found in every part of the specimen, showing that the structure was made up by the material being successively added on previously cooled parts in jets or small streams.

When subjected to chemical tests, iron in a metallic state was found; its composition was that of cast iron containing carbon, without any trace of nickel. The other constituents were in an oxydized state, excepting a minute portion of sulphur compound, while traces of copper were found in every part of the substance.

It was composed of silica, proto-peroxyd of iron, lime, alumina, magnesia, sulphur, copper, which are the bodies present in the slags of copper furnaces. Like those slags, the specimen examined was readily decomposed by acids, in which the bases dissolve with a portion of silicic acid; *no natural minerals remain.* The composition of this compound, the presence of crude iron with its carbon constituent, and its mechanical structure, offer positive evidence of its being of artificial origin.

In the absence of nickeliferous or chromiferous iron, simple minerals of the magnesian class, and a natural internal structure, we have the negative evidence. Both leave no doubt that this body is a furnace product formed in smelting copper ores, or iron ores containing copper.

3. *On the Volcano of Kilauea, Hawaii;* by the Rev. TITUS COAN. (From a letter to J. D. DANA, dated Hilo, Sept. 1, 1857.)—I was at Kilauea with the younger Binghams and others in June last. Pele was rather quiet. The latest change is the subsidence of the vast dome, some 300 feet high and two miles in circuit, which covered the area of the ancient fire-lake, Halemaumau. All that area is now a deep basin, encircled by a rim consisting, in some places, of a bold perpendicular precipice, and in others of an inclined plane of unequal angles, rent into numerous yawning fissures and strewed with immense masses of debris. The bottom of this basin is rent and smoking, and studded with a few cones. Near the centre, and enclosed by a jagged rim from 20 to 50 feet high, is the lake of fire, which has burnt from time immemorial. It is about 100 feet below the rim and some 500 feet in diameter. When our party approached it, there was very little action; but in about half an hour, mother Pele, as if to give us a special benefit, began to fire up in earnest; the great cauldron boiling furiously on the southern side; the glaring fusion rolled in a fiery wave over the black and hardened crust which covered the lake like ice, breaking it down by sections, and tilting it at an angle of 30°, carried it under the burning flood, until the whole surface of the lake was aglare and all boiling together with vehement heat. This whole process did not occupy more than three minutes. Not a square inch of hardened crust remained. All was glaring fusion; and so intense was the radiating heat, that our whole party were driven precipitately back from our point of observation on the windward or northeastern bank, and more than 100 feet above the lake. No person could have approached the southern bank. After a little season, all was quiet again and the surface of the lake blackened and crusted over; Pele had dropped her curtain. These scenes were repeated in the night, as we could see from the great brilliancy occasionally displayed.

Mr. Coan, in the same letter, states it as his opinion based on his survey of the region, that the lavas of the last summit eruption of Mount Loa, which began in 1855, and continued on for fifty miles, all flowed from a *single* opening,—that of the first great outbreak.

4. *Earthquakes.*—About four o'clock on the morning of October 8, a shock of an earthquake at St. Louis, Missouri, "made the more substantial buildings tremble." Seven minutes later there was another shock. These shocks were felt at Springfield, Illinois, and elsewhere. At Centralia, Illinois, there were three distinct shocks at intervals of five minutes, at about the same hour in the morning, the first of the three being powerful enough to throw down chimneys.

On the 23d of October, soon after three o'clock in the afternoon, an earthquake shock was felt at Buffalo, N. Y. It was also perceived to the westward in Ohio, at Dayton, Forestville, etc.

Another shock occurred at Charleston, S. Carolina, on the morning of the 19th of December, about nine o'clock.

It is much to be desired that some person in the region of these earthquakes should collect all the information respecting them, especially with reference to the time and intensities of the shocks at different places,—these times accurately determined, being the data necessary for deducing the direction from which the earthquake came, its course, breadth, and progress, and the intensities, giving the point of greatest action.

5. *Tables of the Division of Mankind into Races, Branches, Families and Nations, with an approximative statement of the Population;* by M. D'OMALIUS D'HALLOY, (Bull. de l'Acad. Roy. Belgique, tome xxiii, 1856, p. 812.)

I. *Division into Races and Branches.*

WHITE RACE.—European branch,	289,586,000		
Aramean "	50,390,000		
Scythian "	30,747,000	=	370,723,000
YELLOW RACE.—Hyperborean branch,	160,000		
Mongolian "	7,000,000		
Sinic "	338,300,000	=	345,460,000
BROWN RACE.—Hindoo branch,	171,100,000		
Ethiopian "	8,300,000		
Malay "	25,600,000	=	205,000,000
RED RACE.—Southern "	9,200,000		
Northern "	400,000	=	9,600,000
BLACK RACE.—Western "	56,000,000		
Eastern "	1,000,000	=	57,000,000
HYBRIDS, Mulattos, Zambos, &c.			12,217,000
Total,			1,000,000,000

II. *Subdivision of the* WHITE RACE *into Families and Nations.*

1. *European Branch.*

TEUTONIC FAMILY.			
Germans, including the Dutch,	54,000,000		
Scandinavians.—Swedes,	3,634,000		
Norwegians,	1,563,000		
Danes,	1,709,000		
English, including the Scotch,	38,014,000	=	98,920,000
CELTIC FAMILY.			
Cymry.—Welsh,	650,000		
Bretons,	1,000,000		
Gaels.—Irish,	9,600,000		
Highlanders,	500,000	=	11,750,000
LATIN FAMILY.			
French,	39,900,000		
Spaniards, including the Portuguese,	22,865,000		
Italians,	26,160,000		
Wallacks,	7,095,000	=	96,020,000
GREEK FAMILY.			
Greeks,	2,990,000		
Albanians,	1,480,000	=	4,470,000
SLAVIC FAMILY.			
Russians, including the Rusniaks and Cossacks,	49,874,000		
Bulgarians,	3,387,000		
Servians,	5,500,000		
Slovenians,	1,306 000		
Wends,	142,000		
Chechs.—Bohemians,	3,144,000		
Moravians,	1,000,000		
Hanaks,	280,000		
Slovaks,	2,400,000		
Poles,	9,304,000		
Lithuanians.—Lithuanians, properly,	1,217,000		
Lettish,	872,000	=	78,426,000
Total,			289,586,000

2. *Aramean Branch.*

Basque Family.			
Basques,			775,000
Lybian Family.			
Berbers.—Amazirghs,		4,700,000	
Kabyles,		1,500,000	
Tuaries,		300,000	
Egyptians.—Copts,		150,000	
Fellahs,		1,500,000 =	8,150,000
Semitic Family.			
Arabs,		14,650,000	
Jews,		4,074,000	
Syrians,		500,000	
Maltese,		106,000 =	19,330,000
Persian Family.			
Tajiks,		8,775,000	
Afghans.—Afghans, properly,		3,500,000	
Beloochis,		1,500,000	
Patans,		5,000,000	
Kurds, including the Lures,		1,500,000	
Armenians,		1,228,000	
Ossetians,		32,000	
Georgians, including the Mingrelians and Lazians,		600,000 =	22,135,000
Total,			50,390,000

3. *Scythian Branch.*

Circassian family.			
Cherkessians,		800,000	
Chetchents,		200,000	
Lesghians,		500,000 =	1,500,000
Magyar family.			
Magyars,			5,000,000
Turkish family.			
Osmanli,		9,500,000	
Turcomans,		1,500,000	
Tarekamehs,		1,000,000	
Nogai,		1,470,000	
Kirgis,		1,000,000	
Usbeks,		5,500,000	
Alatys,		30,000 =	20,000,000
Finnish family.			
Finns of Siberia.			
Telecuts,	1,000		
Sagnia; Kachints, etc.	20,000		
Woguls,	12,000		
Ostiaks,	103,000 =	136,000	
Finns of Eastern Russia.			
Bashkirs,	392,000		
Teptiairs,	104,000		
Metcheriaks,	80,000		
Chcuvasehs,	430,000		
Cheremisses,	165,000		
Morduins,	480,000		
Permiaks,	52,000		
Sirijänes,	71,000		
Votiaks,	191,000	1,965,000	
Finns of the Baltic.			
Livonians,	2,000		
Esthonians,	654,000		
Kyrials; Ymes; Quaines,	1,490,000	2,146,000 =	4,247,000
Total,			30,747,000

III. *Subdivision of the* YELLOW RACE *into branches, families and nations.*

HYPERBOREAN BRANCH.

Lapponic family,		9,000	
Samoyedic "		15,000	
Yenisei "		28,000	
Yukagir "		3,000	
Koriak "	Koriaks,	8,000	
	Chutchis,	2,000	
Kamtschatkan family,		5,000	
Esquimaux "	Namollos,	2,000	
	Chugassics,	3,000	
	Kuskovintzes,	7,000	
	Aleouts,	3,000	
	Esquimaux,	20,000	
	Greenlanders,	5,000	
Kurilian family.—Aïnos,		40,000 =	160,000

MONGOLIAN BRANCH.

Yakut family.—Yakuts,		90,000	
Mongolian family.—Kalmucks,		170,000	
	Mongolians,	2,560,000	
	Burattish,	120,000	
Tungusian family.—Tungusians,		60,000	
	Mantchurians,	4,000,000 =	7,000,000

SINIC BRANCH.

Chinese family,		282,000,000	
Corean "		6,000,000	
Japanese "		25,000,000	
Anamitic "		12,000,000	
Siamese "		4,300,000	
Peguan "		500,000	
Birman "		2,500,000	
Thibetan "		6,000,000 =	338,300,000
	Total,		345,460,000

IV. *Subdivision of the* BROWN RACE *into branches, families and nations.*

HINDOO BRANCH.

Hindoo family.	Hindees; Guzurats; Mahrattas; Bengalees; Oriyas; ? Taiganes,	111,100,000	
Malabar family.	Telingas; Carnatics; Tamils; ? Singalese; ? Gonds; ? Bhills; ? Paharias; ? Kacharis, etc....	60,000,000 =	171,100,000

ETHIOPIAN BRANCH.

Abyssinian family.	Barabras; Tibboos; Abyssinians; Galias, etc.	4,300,000	
Fellan family.—Fellahs; Ovas, etc.		4,000,000 =	8,300,000

MALAY BRANCH.

Malay family.	Malays; Battas; Javanese; Macassars; Bugis; Turajas; Dayaks; Bissayis; Tagalis, etc.	24,600,000	
Polynesian family.	N. Zealanders; Tongas; Bougainvillians; Cook's Islanders; Tahitians; Paumotuans; Marquesans; Sandwich Islanders; Caroline Islanders; Mulgravians,	1,000,000 =	25,600,000
	Total,		205,000,000

V. *Subdivision of the* Red Race *into branches and families.*

Southern branch.			
Aztec family,	4,435,000		
Maya "	300,000		
Quichua "	2,620,000		
Antisian "	100,000		
Araucanian family,	340,000		
Pampean "	250,000		
Chiquitean "	20,000		
Moxean "	30,000		
Guaranian "	1,105,000	=	9,200,000
Northern branch.			
Floridian family,	70,000		
Iroquois "	5,000		
Lenape "	40,000		
Athabascan "	40,000		
Sioux "	35,000		
Pawnees "	80,000		
Kolushen "	50,000		
Wakash "	20,000		
Californian "	60,000	=	400,000
Total,			9,600,000

VI. *Subdivision of the* Black Race *into branches, families and nations.*

Western branch.		
Caffre family. Hottentot " Negro "	A large number of nations, of whom the most are unknown,	56,000,000
Eastern branch.		
Papuan family.	Feejeeans; New Caledonians; New Hebrideans; Saloinon Islanders; Papuas,	1,000,000
Andamanian family,	Andamans of the Andaman Isl., Indo-China, New Guinea, New Holland, Van Diemens Land,	
	Total,	57,000,000

6. *Artesian Wells in Sahara*, (Athen., No. 1562).—The *Moniteur Algérien* brings an interesting report on the newly-bored Artesian wells in the Sahara Desert, in the province of Constantine. The first well was bored in the Oasis of Oued-Rir, near Tamerna, by a detachment of the Foreign Legion, conducted by the engineer, M. Jus. The works were begun in May, 1856, and, on the 19th of June, a quantity of water of 4,010 litres per minute, and of a temperature of 21° Réaumur, rushed forth from the bowels of the earth. The joy of the natives was unbounded; the news of the event spread towards the South with unexampled rapidity. People came from long distances in order to see the miracle; the Marabouts, with great solemnity, consecrated the newly-created well, and gave it the name of "the well of peace." The second well, in Temakin, yielded 35 litres, of 21° temperature, per minute, and from a depth of 85 metres; this well was called "the well of bliss." A third experiment, not far from the scene of the second, in the Oasis of Tamelhat, was crowned with the result of 120 litres of water per minute. The Marabouts, after having thanked the soldiers in the presence of the whole population, gave them a banquet, and escorted them in solemn

procession to the frontier of the Oasis. In another Oasis, that of Sidi-Nached, which had been completely ruined by the drought, the digging of "the well of gratitude" was accompanied by touching scenes. As soon as the rejoicing outcries of the soldiers had announced the rushing forth of the water, the natives drew near in crowds, plunged themselves into the blessed waves, and the mothers bathed their children therein. The old Emir could not master his feelings; tears in his eyes, he fell down upon his knees, and lifted his trembling hands, in order to thank God and the French. This well yields not less than 4,300 litres per minute, from a depth of 54 metres. A fifth well has been dug at Oum Thior, yielding 108 litres per minute. Here a part of the tribes of the neighborhood commenced at once the establishment of a village, planting at the same time hundreds of date-palms, and thus giving up their former nomadic life. The last well is that of Shegga, where soon an important agricultural centre will spring up. There is no doubt but that these wells will work in these parts a great social revolution. The tribes which, after the primeval custom of their ancestors, kept wandering from one place to another, will gather round these fertilizing springs, will exchange the herdsman's staff for the plough of the farmer, and thus take the first steps towards a civilization, which, no doubt, will make rapid progress in Northern Africa.

7. *Ascent of Chimborazo*, (Ed. N. Phil. J., vi, 370).—The *Echo du Pacifique* of the 3d January, gives the following account of an ascent of Chimborazo, made on the 3d November, 1856, by a French traveller, M. Jules Remy, accompanied by M. Brenchley, an English traveller.

"On the 23d of June, 1802, the illustrious Humboldt, accompanied by his friend Bonpland, made the first attempt to ascend Chimborazo. On account of a pointed rock, which presented an insurmountable barrier, they were unable to ascend above 5909 metres of the mountain, then regarded as the highest in the world, and which still occupies a principal place among the colossi of America.

"Thirty years later, on the 16th of December, 1831, M. Boussingault, after a long and skillful examination of the Cordillera of the equator, endeavored to accomplish the ascent in which his predecessor had failed. He reached the enormous height of 6004 metres, that is to say, 95 metres higher than the others; but he was arrested by rocks as they had been, and could not get beyond this limit, which was then the most elevated point ever attained by man on mountains.

"The accounts of these famous travellers had deprived us of all hope of reaching a height so considerable; but, after having observed the snowy and rounded summit of Chimborazo from Guayaquil, we could not help thinking that it was accessible from some point or other. M. Brenchley and myself were thus led to form the design of attempting a third ascent.

"On the 21st of July, 1856, as we crossed the plateau of the Andes on our way to Quito, we halted at the foot of this stupendous mountain. We employed two days in studying its outlines from a distance, with the view of discovering any peculiar places on the surface of its gigantic dome which might afford us a passage.

"The route followed by MM. Humboldt and Boussingault, seemed to us at first to be greatly the most easy and desirable on account of its

regular declivity; but the barrier of rocks, which we readily distinguished, presented no outlet to the eye. When we had made nearly the entire circuit of this mighty mountain, and without success, we resumed our journey towards Quito, reserving the execution of our plan till we should be better fortified against the rigorous climate of the higher Cordilleras.

"After visiting Pichincha, Cotopaxi, and other giants of the Andes, we again found ourselves, on the 2d of November, at the foot of Chimborazo. We pitched our camp at a height of 4700 metres, a little below the line of perpetual snow, in a valley between Arenal and the point where the Riobamba route separates from that of Quito. We intended to spend the following day in collecting plants and hunting deer and birds, endeavoring, at the same time, to determine beforehand the places which might afford us the most easy access to the summit.

"We took up our quarters under a huge inclined rock, which afforded us sufficient protection against the northwest wind, but gave us no shelter in the event of rain. Rain had fallen in the afternoon. The weather cleared at night-fall, the sky became sprinkled with myriads of stars, and Chimborazo was delineated, in all its splendor, on the azure and sparkling vault of the firmament.

"On the morning of the 3d of November, at five o'clock, when day had not yet dawned in the equinoctial regions, we left our camp in charge of our people, and departed on our exploring expedition, carrying with us a coffee-pot, two thermometers, a compass, matches, and tobacco. A steep hill, sandy and rough with pebbles, which separated us from the perpetual snow, occasioned us so much fatigue at our outset, that two of the natives who accompanied us became discouraged and turned back.

"When we had surmounted this hill, we descended on some soft sand to the bottom of a valley, which we followed, and from the extremity of which, we distinguished very clearly the summit of the mountain, entirely free from snow.

"After walking half an hour on the snow, vegetation suddenly ceased, and we saw no other living thing but two large partridges, and on the rocks a few lichens of the families Idiothalamus and Hymenothalamus. At this point of our ascent we collected some dry branches of chuquiragua, and made a bundle of them which we tied to our backs. We had still to scale an immense rock of trachyte, from the top of which the summit of Chimborazo appeared to us so near, that we thought we could reach it in half an hour.

"Our ascent was so rapid, that we were soon obliged, from fatigue, to make frequent stoppages to recover our breath. Thirst also began to be severely felt, and in order to moderate it, we almost always kept snow in our mouths. But we felt no symptoms of illness or any morbid affection, such as is spoken of by the majority of travellers who have ascended high mountains.

"After halting a few seconds, without even seating ourselves, we again started not only with renewed ardor, but even a kind of furious determination inspired by so near a view of the summit. It appeared evident to us, by this new instance confirming so many previous ones, that at these heights the atmospheric column is still sufficient to prevent any impediment to respiration, and that the shortness of breath and organic

affections which are so generally complained of at considerable elevations, must be ascribed to some other cause.

"Always rapidly ascending, we now began to overlook the peaks of the Cordilleras, and to discover a distance furnished with immense valleys, when some light vapors, which at first appeared only like spiders webs on the sides of the mountains, soon began to detach themselves in the form of white flakes, stretching nearer and nearer to each other, till they at last arranged themselves like a girdle along the horizon.

"All of a sudden, about eight o'clock, this curtain enlarged itself, and approached Chimborazo; then in a few minutes, it mounted to us, thin at first, but becoming perceptibly more dense. We no longer could perceive the summit. We continued, however, to mount upwards, enticed by the hope of attaining our object much more easily than we had supposed on leaving our encampment.

"The fog continued to increase; we could not see twenty paces from us. At half-past nine, it had become so thick that it was almost as dark as night at the distance of a few meters. Confident of finding our footsteps again to guide our descent, we travelled on with additional stubbornness; but we had every moment to examine the compass, in order to avoid a precipice which we had left on our right before reaching the terminal depression by which we resolved to gain the summit.

"It seemed to us that the declivity became less steep, we breathed more freely, and walked with less effort. Some dull detonations began at intervals to be heard in the distance. At first we ascribed them to the explosions of Cotopaxi; but soon reverberating peals, such as are heard only in the vicinity of the equator, convinced us that thunder was rolling in the lower regions. A terrible storm was in preparation.

"In the fear that the hail or snow would efface the marks of our feet, and thereby expose us to the risk of losing ourselves in the descent, we determined, with regret, to halt for a while. We hastened to kindle our chuquiragua wood, in order to melt the snow in our coffee-pot. At ten o'clock, the thermometer which, at five feet above the snow, indicated 1·7, was plunged in boiling water where the mercury stood at 77·5.

"At five minutes past ten, our observations terminated, and we began to descend with giant strides in order to regain our encampment as speedily as possible. We arrived there in the midst of the thick fog about an hour after noon. The thunder rolled almost without interruption, the flashes of lightning describing dazzling zigzags around us, never seen elsewhere so distinctly defined except in pictures.

"About three o'clock, a fearful tempest of rain, hail, and wind assailed us under our rock. It continued throughout a part of the night with a fury which seemed as if it could never be allayed. We were literally lying in water. On the morrow, at day break, our eyes rested everywhere on a vast field of hail.

"Certain indications of another tempest made us abandon the idea of trying again the ascent of Chimborazo, which we henceforth regarded as quite impracticable. We made all haste to break up our camp and make for Guaranda, where we arrived about three o'clock, travelling through a cold and dense fog, which prevented us for that day admiring one of the most beautiful views in the world.

"When we calculated our observations, we were not a little surprised to find that we had reached the summit of Chimborazo without being aware of it. According to personal researches, made at first in the Archipelago of Hawaii, and afterwards repeated among the Cordilleras of the equator, the co-efficient of the sum of degrees or fractions of a degree in the centigrade thermometer, reckoning between the point to which the mercury rises when the instrument is immersed in boiling water, and the boiling point of water at the level of the sea, is found to be 290·8; that is to say, each degree below 100 indicates a difference of level equal to 290·8 meters, or about 29 meters for the tenth of a degree, hence the formula

$$x = (100 - B)\ (290{\cdot}8)$$

which gives us 6543 meters for the absolute vertical height we had reached on Chimborazo. This figure places us quite on the summit, the altitude of which, above the sea level, according to Humboldt's triangulations, is 6544 metres. But whatever degree of confidence may be conceded to our calculations, the unquestionable fact resulting from our ascent is, that the summit of Chimborazo is accessible."

8. *New Electrotype Processes*, (Proc. Brit. Assoc., in Edin. N. Phil. Jour., Vol. vi, No. 2, p. 306).—M. L'Abbe Moigno read a paper upon *Three New Electrotype Processes*, and exhibited specimens of considerable interest. The first of these improved processes consists in the employment of platina wires instead of copper, and of making a skeleton figure resembling roughly the outline of the cast sought to be obtained, by means of which, according to M. Lenoir's process, busts, statues, and groups can be produced in full relief by a single operation. The second of these consists in M. Oudrey's process for galvanizing or coppering iron and cast-iron to any thickness required without the cyanid bath. He added remarks upon its employment in commerce and in the navy. The process was not fully communicated, as it is commercially desirable to keep it a secret, but sufficient was communicated to show that the cyanid bath, which is not only expensive but dangerous, can be dispensed with, and that the present system, according to which there is a great waste of material, is avoided, although the substance that is placed upon the iron to induce the deposit of the copper is not stated. The last branch of the paper treated of Messrs. Christofe and Bouillet's process for strengthening electrotypes, the principle of which is to leave an opening in the back of the thin electrotype obtained by precipitating, and to put into it various little pieces of brass, which, on being melted with an oxyhydrogen blast, become diffused all over the interior surface of the copper without injuring it in any way, and thereby impart to it the strength of cast-iron.

9. *On a new method of Refining Sugar;* by Dr. Daubeny, (Proc. Brit. Assoc., from Edin. N. Phil. Mag., vi, 304).—Dr. Daubeny gave an account of *a new method of refining sugar*, conducted at Plymouth by Mr. Oxland, and known by his name. It consists in the adoption of the superphosphate of alumina in conjunction with animal charcoal, as a substitute for the albumen usually employed for that purpose. In both cases the object is to separate and carry down the various impurities which color and adulterate the pure saccharine principle present in the syrup expressed from the cane or other vegetable which supplies it. As, how-

ever, bullock's blood is the material usually procured for the purpose of supplying the albumen, a portion of uncoagulated matter, together with certain salts, is left in the juice in the ordinary process of refining, which impairs its purity and promotes its fermentation—thus occasioning a certain loss of saccharine matter to result. Nothing of the kind happens when the superphosphate is substituted, and so much more perfect a purification of the feculent matters, under such circumstances, takes place, that several varieties of native sugar, which, from being very highly charged with feculent matters, are rejected in the ordinary process of refining, are readily purified by this method. The employment of superphosphate of alumina also gets rid of so much larger a portion of the impurities present in the sugar, that much less animal charcoal is subsequently required for effecting its complete clarification than when bullock's blood has been resorted to. The quantity of superphosphate necessary for effecting the object is, for ordinary sugars, not less than twelve ounces to the ton; whereas, for the same quantity, as much as from one to four gallons of bullock's blood is found to be required. Dr. Daubeny suggested that this reagent might be advantageously resorted to not only in the purification of sugar, but also in other processes of the laboratory, when the removal of foreign matters, intimately mixed with the solution of a definite component, becomes a necessary preliminary in its further examination.

10. *Report on the Development of Heat in Agitated Water;* by Mr. G. Rennie, (Proc. Brit. Assoc., Athen., No. 1559).—Mr. Rennie, in alluding to his former papers on the subject, read before the Section last year, at Cheltenham, stated that the subject of the mechanical or dynamic force required to raise a given quantity of water one degree of Fahrenheit had long been the object of the research of philosophers, ever since Count Rumford, in his celebrated experiments on the evolution of heat in boring guns when surrounded by ice or water, proved the power required to raise one pound of water one degree, and which he valued at the dynamic equivalent of 1,034 lbs. M. Moya was the first who announced that heat was evolved from agitated water. The second was Mr. Joule, who announced that heat was evolved by water passing through narrow tubes, and by this method each degree of heat required for its evolution a mechanical force of 770 lbs. Subsequently in 1845 and 1847 he arrived at a dynamical equivalent of 772 lbs. These experiments have since been confirmed by other philosophers on the Continent. In the present paper Mr. Rennie stated that his attention was called to the subject by observing the evolution of heat by the sea in a storm, by the heat from water running in sluices. He, therefore, prepared an apparatus similar to a patent churn, somewhat similar to that adopted by Mr. Joule, but on a large scale. In the first case he experimented on fifty gallons, or 500 lbs. of water, inclosed in a cubical box, and driven by a steam engine instead of a weight falling from a given height, as in Mr. Joule's experiment; secondly, on a smaller scale, by 10 lbs. of water inclosed in a box. The large machine or churn was driven at a slow velocity of eighty-eight revolutions per minute, and the smaller machine at the rate of 232 revolutions per minute, so that the heat given off by the water in the large box was only at the rate of three and a half degrees per hour, including

the heat lost by radiation; whereas the heat evolved by the ten gallons of water contained in the small box agitated at 232 revolutions was fifty-six degrees Fahrenheit per hour. Thus the temperature of the water in the large box was raised from 60 degrees to 144 degrees, and the temperature of the water in the small box to boiling point. As an illustration, an egg was boiled hard in six minutes. The mechanical equivalent in the first case was found to approximate nearly to that of Mr. Joule, but in the latter case it was considerably above his equivalent, arising, very probably, from the difficulty of measuring accurately the retarding forces.

11. *Fossils of South Carolina;* by M. Tuomey and F. S. Holmes.—Along with Nos. 13, 14, 15 of this fine work,—the most beautiful in style of any work on American Palæontology,—we have received the following circular from Professor Holmes, and have the pleasure of adding that the Legislature has authorized its continuation. There will be two more volumes issued, one on the Eocene and one on the Post-pleiocene. The volume just closed contains figures and descriptions of 203 species; 42 per cent. are still living.

Circular.—We have great satisfaction in announcing to the subscribers the publication of our fifteenth and last number. This completes one entire volume, devoted to the Pleiocene Fossils of South Carolina.

In the accomplishment of this cherished design, we have labored assiduously for more than five years, and expended a large sum of money. The receipts will apparently do no more than defray actual expenses; but we do not repine, being recompensed for our toils by the success of publishing a scientific work at home, in the best style of art. To the accomplishment of this object, all our endeavors have been directed.

Should the Legislature continue the subscription in this State, it is proposed to proceed with the work and publish one volume on the Post-Pleiocene, and one on the Eocene Fossils. Should this be the final determination, due notice of our purpose will be given in a prospectus; and we shall hope to receive a continuation of the patronage so liberally given to the first volume.

12. *On the Direction and Velocity of the Earthquake in California of January* 9, 1857; by Dr. John B. Trask.—The earthquake which occurred in various parts of this State on the morning of the 9th of January last, excited at the time considerable attention. This arose from two causes; first, from the varied reports that appeared on the following day through the press of this city, detailing its occurrence in remote mountain towns, and for which there was no foundation; secondly, from the great extent over which the commotion was felt, as was subsequently proved.

Immediately following the occurrence of the phenomenon, letters were addressed to all the principal towns between Mariposa and Downieville, east of the valleys, for the purpose of learning how far the shocks may have extended eastward of this city. These letters were forwarded by the Pacific Express Company to their agents, and through them answers were returned in every case but two through the same source. From the facts thus obtained, it was found that in no locality east of the foothills, *was any shock felt whatever on that day or night.*

Another report, equally unfounded, reached us on the arrival of the steamer from the southern coast, to the effect that several houses had

been demolished in San Diego from its violence, while the facts in the case are, *that the steamer left that port twenty-four hours before the shock occurred there.*

This earthquake, or more properly speaking, the series of shocks that began on the night of the 8th in this city, and which continued in the south part of the State during the following day and night of the 9th, was probably the most extensive of any on record on this portion of the Pacific coast, excepting, perhaps, that of the wave of the Simoda earthquake in December, 1854. The linear distance over which we are able to trace its course, amounts to six hundred and two miles, and its breadth, so far as now ascertained, is two hundred and ninety miles. It has all the appearance of having been the terminal movement of some more violent commotion at a distance from our coast.

From the best evidence attainable at present, it seems to have had its origin to the west and travelled in an easterly direction. This is conclusively proved from the fact that it was felt earlier at San Francisco, than at any other locality east of this city within the State. We have no record as yet of its occurrence along the coast of Mexico or Oregon.

I have been able to determine, with considerable accuracy, the period of time, at which the shock between eight and nine o'clock on the morning of the 9th took place, at four localities east of the city of San Francisco, in this State; the shock at that hour seems to have been more generally noticed than those which either preceded or followed it, here or elsewhere, though at this city, it was much less marked than the shocks at $1^h\ 33^m$, $4^h\ 15^m$, and 7^h, these three latter occurring at those hours of the morning when most persons are sleeping. The shock at 7^h produced a circular motion in the pendulum, the diameter of which was about five inches. The oscillations of the pendulum in all the others were in an easterly and westerly direction.

The precise period of time at which the shock took place at San Francisco, between eight and nine o'clock, is determined by the stopping of a time-piece belonging to J. W. Tucker, whose rate of error was three seconds fast. The time at San Diego was furnished by Mr. Cassidy of the army, and that of the Tejon Reserve is by persons at that post. To private gentlemen at Sacramento and Stockton we are indebted for the time at those places. The accompanying table of latitudes and longitudes, of localities named, gives the hour at which the shock took place at each; the difference or elapsed time, from which the velocity was deduced, are the mean times corrected for the places named, the time as given above being taken as the standard at San Francisco.

It is proper here to state that three minutes four seconds was the greatest error in time found, and the least was twenty-two seconds:—

	Lat.	Lon.	Time of shock.	Elapsed time.	Velocity.
	° ′	° ′	*h. m. s.*	*m. s.*	*miles.*
San Francisco,	37 48	122 25	8 13 30	0 00	0 0
Sacramento,	38 32	121 23	8 20 00	7 30	6 6
Stockton,	37 52	121 34	8 23 00	9 30	6 5
Tejon,	35 00	118 46	8 45 00	32 30	6 0
San Diego,	32 42	117 13	8 50 00	36 30	7 0

The velocity is given in miles per minute; and by dividing the sum of the same by their number, it will be found that the movement of the wave at that time averages a fraction over 6·2 miles per minute.

The results obtained from the above data approximate closely to the deductions of Prof. Bache on the wave which reached our shores resulting from the earthquake at Simoda on the 23d of December, 1854, and which will be found in a paper read by that gentleman at the meeting of the American Association for the Advancement of Science, during the early part of last year.

From the facts before us, there can be little doubt of the direction of the commotion, and that it proceeded from the west, or a little south of that point. The motion of the earth, as described at the different localities at which it was felt, with the motion of the pendulum—which was slightly south of a west line—leads to the latter conclusion. Time is an important element in aiding us to form correct conclusions regarding their phenomena, and it is to be hoped that our friends in different parts of the State, in reporting the same, will be precise in this particular. Of the incidents attending the shocks, many and varied reports have reached us; and it seems to have acted with greater violence in the vicinity of the Tejon Reserve and upper Tulare county than at any other places. It is most remarkable that so small an amount of intensity was manifested when the area over which it extended is taken into consideration.

The effects were felt in San Francisco several hours before they are reported to have been observed at any other place north or south. They began here at twenty minutes past eleven, on the night of the 8th, and continued till thirteen minutes past eight the following morning—six shocks occurring in the interim; while to the south, the first shock that was noticed at the Tejon was at 6 hours 30 minutes, on the 9th. In Los Angeles they continued at long intervals through the day until 23 hours 30 minutes of the same date. I have learned from persons who were present in Los Angeles at this time, and also at the shock of the 14th of July, 1855, that the severity of the latter exceeded that of the 9th of January last.

13. *Action of Light.*—Niepce de St. Victor, distinguished in photography, has recently made some farther discoveries with regard to the action of light. The question which he has aimed to answer, is this,—Does a body, which has been exposed to light, preserve any of the effect when carried to a dark place? Phosphorescence and fluorescence are well known examples of such an effect; but these are not all. On exposing to the sun's rays an engraving which had been for some days in the dark, one-half covered with an opaque shade, and then applying this engraving to very sensitive photographic paper for twenty-four hours in the dark, the white parts of the portion not covered are reproduced in black; while the same engraving treated thus, without exposure to the light, produces no effect. Pictures on all kinds of paper, wood, ivory, parchment, produce the same result, through in different degrees, but not when on metals. An interposed plate of glass, mica, quartz crystal, or yellow glass colored with oxyd of uranium, prevent the action, as they also do, as recently proved, the phosphorescent property. A coat of collodion

does not interfere with the result, though common varnish does. The effect takes place if the sensitive paper is not in contact with the engraving. A design traced on white paper with a fluorescent liquid, as sulphate of quinine, exposed to the sun and then applied to the sensitive paper reproduces itself much more rapidly and strongly than the white paper; if not first exposed to the sun there is no effect. Luminous lines of phosphorus act without exposure to the sun, but not if glass be interposed.

14. *General View of the Animal Kingdom;* by A. M. REDFIELD. A chart, measuring 5 feet by 4½. New York City and Hartford, Ct.: E. B. & E. C. Kellogg.—This handsome engraved chart is an exhibition of the classification of the Animal Kingdom, by an arrangement of branching spaces divided off according to the classes, orders, families, etc. The four trunks, of the Radiates, Mollusks, Articulates, Vertebrates, with their successive subdivisions sweep gracefully over the chart, without any fanciful shaping out of a tree; and in addition to the lettering of the branches, which is quite detailed, there are well engraved figures of species in large numbers, making an exhibition of the Animal Kingdom of real value for class instruction. The figures among the Mammals include half a dozen species of Monkeys, fifteen or twenty of Carnivora, nearly as many of Ungulata or hoofed Quadrupeds, and so on through all the groups in each of the Sub-kingdoms. The plan is excellent, and it is well carried out by its accomplished authoress. We could wish some small changes made in the classification. But the chart, as it is, will be found of great assistance to the teacher, and should be in all the academies and higher schools of the land. It speaks to the eye, and more impressively than a volume of text.

15. *Reports of Explorations and Surveys to ascertain the most practicable and economical route for a Railroad from the Mississippi River to the Pacific Ocean*, made under the direction of the Secretary of War in 1853–4, according to acts of Congress of March 3, 1853, May 31, 1854, and August 5, 1854. 4to. Vols. 2, 3, 4. Washington, 1855, 1856.—The surveys for the railroad to the Pacific have already been briefly noticed in this Journal. The Reports, before published in brief, are here brought out with full details, and include a large amount of natural science as well as general geographical information. The collections we understand have been great, and now constitute part of the very large cabinet of American Natural History at the Smithsonian Institution. The following are the contents of the volumes here announced.

Vol. II. 1. Report of Explorations near the 38th and 39th parallels of north latitude made by Capt. J. W. Gunnison, Topog. Eng., made by Lieut. E. G. Beckwith, Third Artillery. 128 pp. with plates.—Capt. Gunnison, it will be remembered, died at the hands of the Indians while carrying forward his surveys. Page 94 to 112 are occupied with tables of meteorological observations.

2. Report on the line of the 41st parallel, by Lieut. E. G. Beckwith. 114 pp. with plates: Meteorological observations occupy pages 71 to 95; a Geological Report by James Schiel, M.D., Geologist of the Expedition, pages 96–111, and a letter on Infusorial Fossils by Prof. J. W. Bailey, on pages 111, 112. The paper by Dr. Schiel is illustrated by four lithographic plates containing figures of some Carboniferous and Cretaceous fossils.

3. A Report on the Botany of the two Expeditions by John Torrey and Asa Gray, covering pages 119 to 132 and illustrated by ten plates.

4. Synopsis of a Report of the Reconnaissance on the route from Puget Sound via South Pass to the Mississippi river, by Fred. W. Lander, Civil Engineer. 44 pages.

5. Report of Exploration on the route near the 32nd parallel from the Red River to the Rio Grande, by Brevet Captain John Pope, Corps Topog. Eng. 156 pages. It concludes with a Report on the Botany of the Expedition by John Torrey and Asa Gray, pp. 157–178, with ten plates.

6. Report on the Geology of the route near the 32nd parallel, by Wm. P. Blake. 50 pages, with a map, and a section across the mountains.

7. Report of Explorations near the 32nd parallel between Doña Ana on the Rio Grande and Pimas Villages on the Gila, by Lieut. John G. Parke. 28 pages.

8. Extract from a Report of a Military Reconnaissance made in 1846, 1847, by Lieut. Col. W. H. Emory. 22 pages.

Vol. III. Report of Lieut. A. W. Whipple, Topog. Eng., on the route near the 35th parallel. 36, 136, and 77 pages, with plates.

2. Report upon the Indian Tribes, by Lieut. A. W. Whipple, Thomas Ewbank, Esq., and Prof. Wm. W. Turner. 127 pages, with plates of Indian relics, vocabularies of the languages, etc.

3. Report on the Geology of the route by Wm. P. Blake, 175 pages, including, with Mr. Blake's account, the Report of Jules Marcou, and descriptions of the Fossils by James Hall, and illustrated by maps, sections, and plates of fossils.

Vol. IV. 1. Report on the Botany of the Expedition under Lieut. Whipple. Embracing—1. General descriptions of the Botanical character of the country, by J. M. Bigelow, M.D.; 2. Description of Forest Trees, with a map illustrating Geographical Distribution, by J. M. Bigelow, M.D.; 3. Description of the Cactaceæ, with 24 plates, by G. Engelmann, M.D., of St. Louis and J. M. Bigelow, M.D.; 4. Descriptions of the General Botanical Collections, with 25 plates, by John Torrey; 5. Description of the Mosses and Liverworts, with 10 plates, by W. S. Sullivant.

2. Zoology of the Expedition. Field notes and explanations, by C. B. R. Kennerly, M.D. (17 pages).—The remainder of the Zoological Report is deferred to another volume; and we doubt not that the volume will be one of great scientific value.

3. Appendices to the volume, including astronomical, magnetic, and climatological observations.

We rejoice that the General Government has carried forward so liberally publications like the above, illustrating the geography, history and natural productions of the country. And we trust the work may go on under the same auspices, and on a scale no less liberal, until the land is thoroughly searched out, and a series of volumes covering fully the great subjects shall be in all its prominent libraries.

16. *Report of the Geological Survey of the State of Vermont;* by Edward Hitchcock, State Geologist. 12 pp., 8vo. Montpelier: 1857. —The Vermont geological survey has been in progress during the past year under the direction of Prof. Hitchcock. This Report is a brief statement with respect to the funds and means necessary for the continuation of the survey.

17. *Flora from the Appalachian Coal-field;* by JAMES P. KIMBALL of Salem, Mass. An Inaugural Dissertation for the degree of Doctor of Philosophy, addressed to the Philosophical Faculty of the University of Göttingen. 32 pp., 8vo, with 3 lithographic plates.—A number of species of fossil plants from Pennsylvania are described and figured in this pamphlet, pertaining to the genera Neuropteris, Alethopteris, Hemitelites, Sigillaria, Syringodendron and Lepidodendron.

18. *On the Determination of Altitudes, from Observations taken with the Barometer;* being Chapter VI and Appendix E of the Report of Lieut. HENRY L. ABBOT, Corps of Topog. Eng., upon Explorations for a Railroad Route from the Sacramento Valley to the Columbia River, made by Lieut. R. S. Williamson, Topog. Eng., assisted by Lieut. Henry L. Abbot, Topog. Eng., in 1855.

19. *Uber neue Echinodermen des Eifeler Kalkes;* von JOH. MÜLLER. 30 pp., 4to, with 4 plates. Berlin. From the Transactions of the Königl. Akad. Wissensch. zu Berlin, 1856.—Prof. Müller has described in this paper several of the Crinoids from the Eifel (Devonian) limestone. The species are of the genera *Taxocrinus, Hexacrinus, Trichocrinus* (Müller), *Nanocrinus* (*Müller*) and *Poteriocrinus*. Besides these, *Lepidocentrus Eifelianus* (Müller) is an Echiniform species from the Eifel.

20. *Topographical and Geological Report of Chester Co., Pennsylvania;* by W. D. HARTMAN, M.D. 12 pp., 8vo, with a colored geological map. Philadelphia, 1857. Extracted from the Transactions of the Pennsylvania State Medical Society.—Chester County, Pennsylvania, has great interest to the mineralogist, on account of the large number of fine minerals it affords in connection mostly with its serpentine and limestone and lead and copper veins. This paper, though mainly medico-topographical, contains valuable mineralogical and geological observations; and a geological map gives it special value.

21. *Determinative Mineralogy:* Tables for the determination of minerals by help of simple chemical experiments; by FRANZ VON KOBELL, Prof. of Mineralogy in the University of Munich. Translated from the last German edition and the Author's manuscript notes, and prefaced by a complete treatise on the Use of the Blowpipe, by Professors G. J. BRUSH and S. W. JOHNSON of the Yale Scientific School.—This work—one much needed in the country—we understand will soon be published.

22. *Volcano of Hawaii.*—In the article on page 136 respecting the volcano, Mauna Loa, it is stated that the Rev. Mr. Coan regards the lavas of the late eruption as having flowed from a single opening. It should be added that he supposes this opening may have been a fissure or series of fissures extending ten or fifteen miles from the highest point; but that for the lower forty miles there were no openings supplying the flow of lava.

23. *Organic Morphology.*—The work on Organic Morphology noticed in our last volume at page 443, was by JOHN WARNER.

24. *Chemistry.*—An accomplished student in Science, who has spent several years in the laboratories of Professors Liebig and Agassiz, is desirous of obtaining a situation as Professor of Chemistry, or of this and Natural History combined. Reference may be made to Professors Agassiz or Gray of Cambridge, or to the Editors of this Journal.—EDS.

J. M. Safford and Richard Owen, M.D.: Report of the Mineral and Agricultural Resources of the lands owned by the Hopkins Mastodon Coal and Iron Mining and Manufacturing Company. 52 pp. 8vo, with a map.

The Parish Will Case, before the Surrogate of the City of New York: Medical Opinions on the mental competency of Mr. Parish, by Drs. John Watson, D. T. Brown, M. H. Ranney, Pliny Earle, Luther V. Bell, I. Ray, Sir Henry Holland, Bart. 574 pp. 8vo. New York, 1857.

J. J. Griffin: The Radical Theory in Chemistry. Crown 8vo. *London.* 1857.

R. Bunsen: Gasometry; comprising the leading Physical and Chemical Properties of Gases, together with the methods of gas analysis. Translated by H. E. Roscoe. 8vo, with 58 illustrations. *London.* 1857.

Prof. Allman: British Freshwater Zoophytes. 4to, with 11 colored plates. Published by the Ray Society, London.

The *Ray Society* will also publish A Monograph on the Oceanic Hydrozoa (Jelly-Fishes) by Prof. Huxley; A Monograph of the British Foraminifera, by Prof. Williamson; A General History of the Foraminifera, by Dr. Carpenter; A Monograph of the British Sponges, by J. S. Bowerbank.

Adam White: A Popular History of British Crustacea. 12mo. *London.* 1857.

Charles Pickering: The Geographical Distribution of Plants and Animals. 4to, (vol. xv. of the U. S. Exploring Expedition under Capt. Wilkes). *Boston.* Little, Brown & Co. In cloth $3.00.

Julius Victor Carus: Icones Zootomicæ,—a general and systematic work on the Anatomy of Animals. 1st part, devoted to the Invertebrata, each group being illustrated by very numerous figures on 23 folio plates. The remaining part will contain the Vertebrata. Received too late for a further notice in this number.

Frank Buckland (son of the late Dean Buckland): Curiosities of Natural History. 8vo. *London,* 1857. Bentley.

O. G. Costa: Fauna del Regno di Napoli, etc. Fasc. 96 to 99 have appeared. 152 pp. 4to, with 15 colored plates. *Naples,* 1857.

G. P. Deshayes: Description des Animaux sans vertèbres, découverts dans le bassin de Paris,—a Supplement to Deshayes's former work on the Fossils of the Paris Basin, and comprising a general review of all the known species. p. 161 to 240, 4to, with 10 plates. *Paris,* 1857.

E. Desor: Synopsis des Echinides Fossiles. Livr. 1-4, 4to. 240 pp. 30 plates. *Paris.* 1857.

Faune Française, ou Histoire Naturelle, Générale et Particulière des Animaux qui vivent en France: par Charles Lucien Bonaparte et Victor Meunier. A Prospectus of this "French Fauna" has just been issued. As projected, the work will extend to 24 volumes 8vo, one to be issued every three months, the first, in Jan. 1858. It will contain very numerous wood-cuts, and colored engravings, and will be the most complete and elegant work of the kind that has ever been published. Subscriptions are solicited.

Th. Lacordaire: Histoire Naturelle des Insects—Genera des Coléoptères. Tome IV, 583 pp. 8vo. *Paris,* 1857.

A. Machado: Catalogos de los Peces, etc.: Fishes of Cadiz and Huelva and of the river Guadalquiver. 30 pp. 4to. *Seville,* 1857.

Proceedings Boston Soc. Nat. Hist., Vol. VI. p. 253, Corrosion of living shells. p. 260, Astræa decactis, a new species of coral; *T. Lyman.*—p. 264, Vibrations at the dam at Hadley Falls; *C. Stodder.*—p. 269, Peculiar mode of gestation in the Aspredos or Trompettis, etc.; *J. Wyman.*—p. 274, On a new genus and species of Coral, Syndepas Gouldii; *T. Lyman.*—p. 279, Note on Native Iron of Liberia; *A. A. Hayes.*—p. 281, Larva of a Musca or Œstrus found in the skin of the scalp, face, neck and back of a child; *B. S. Shaw.*—p. 282, On the Fungus, Glæosporium Crocosporum; *C. J. Sprague.*—p. 283, On the copper of Lake Superior; *Dr. Kneeland.*—p. 284, Note on a siliceous calculus taken from the Urethra of an Ox; *Bacon.*—p. 287, On a new species of Coral, Oculina glomerata; *T. Lyman.*

Proceedings of the California Acad. Nat. Sci.—p. 96, A new species of Quercus; *Dr. Kellogg.*—Telluret of Silver in California; *W. P. Blake.*—p. 98, On the Earthquake of Jan. 9, 1857; *J. B. Trask.*—p. 99, On some new microscopic organisms, with a plate; also on some Sertularidæ and Bryozoa; *Dr. Trask,* with two plates.—p. 102, New marine shells of the Sandwich Islands (species of Murex, Purpura, Turbo, Trochus, Pleurotoma, Rissoa,); *Garratt.*